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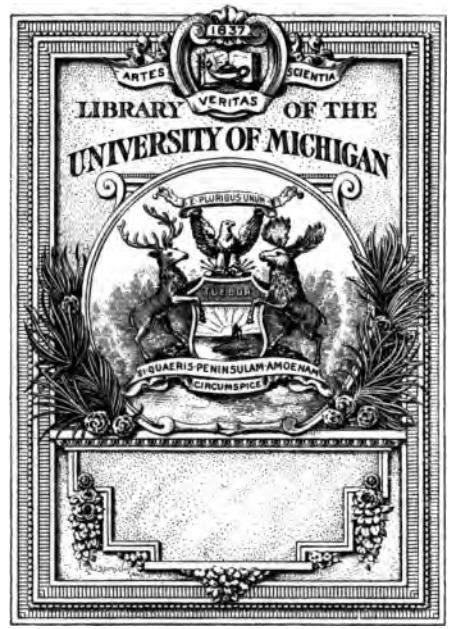
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THE

RETROSPECT

OF

PRACTICAL MEDICINE AND SURGERY,

BEING

A Half-Yearly Journal,

CONTAINING A RETROSPECTIVE VIEW OF EVERY DISCOVERY AND PRACTICAL IMPROVEMENT IN THE MEDICAL SCIENCES.

EDITED BY

W. BRAITHWAITE,

LECTURER ON OBSTETRIC MEDICINE AT THE LEEDS SCHOOL OF MEDICINE, ETC.

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PRACTICAL MEDICINE,

&c., &c.

DISEASES AFFECTING THE SYSTEM GENERALLY.

ART. 1.—ON THE ARREST OF TYPHUS FEVER BY QUININE.

By DR. ROBERT DUNDAS, Physician to the Northern Hospital, Liverpool, &c.

[DR. DUNDAS believes that to some extent his views on this subject have been misapprehended; he does not say that large and repeated doses of quinine will *always* cure or arrest typhus fever; but he does say that where remedies can save a patient, the treatment by quinine is as likely to benefit the patient as any other, and, in the language of Mr. Eddowes, "either cut the fever short or prevent the accession or increase of the more formidable symptoms." Dr. Dundas believes that the ordinary dogma first laid down by Pitcairn, that "you may guide a fever, but you cannot cure it," should in reality be reversed. Dr. Dundas only introduces here one case of his own, the notes of which were taken, and the treatment carried out by Mr. C. J. Evans, now apothecary to the Liverpool Infirmary. Dr. D. proceeds:]

I shall now submit the valuable evidence of Dr. Goolden, who has also touched on some practical points, to which I shall afterwards briefly advert. In answer to my letter, requesting him to communicate to me the result of his experience of the quinine treatment in typhus fever, he replies:—

"I have only to state that it is so satisfactory, that I should not feel myself justified in treating any serious case of typhus without it. Of eight cases admitted under my care in St. Thomas's in one week—five adults and three children—all with the characteristics of typhus gravior, with one exception, the effect was well marked. Each dose of quinine produced a sensible effect; ten grains in solution, every two hours, to an adult, produced some giddiness and deafness in about thirty-six hours, when it was discontinued; and it was only necessary to give a few doses of nitre and a slight aperient, and nothing remained of the fever but slight debility—debility varied according to the previous duration of the disease. In one case in the hospital, there was so much headache and excitement after each dose, that after the third time it was discontinued, but there was no permanent ill effect; and I am satisfied the course is quite safe to adopt.

"I have met with several opportunities of trying it in private practice.

"In one case, a young gentleman had been travelling with his tutor in Germany, and, when at Giessen, was observed to be out of health; he had loss of appetite, shivering, debility, thirst, and feverish nights. Still he was able to make the journey home. When at home, he was attacked with severe typhoid symptoms, and I visited him with his medical attendant. I found him with *a black dry tongue, hot dry skin, petechiæ, pulse* 140 *and full, delirium, deafness, and diarrhœa.* He had been in this state several days, when I suggested the large doses of quinine. After an emetic, he took eight grains every two hours. I saw him after the *fourth dose,* and found him *sensible, but rather deaf, the skin bathed in perspiration, pulse sunk down to* 80, *and the diarrhœa checked.* The medicine was continued during the night, and discontinued the next morning, as the surgeon in attendance had had no experience in the quinine treatment, and did not like to carry it on upon his own responsibility. I saw him two days afterwards, and found that he had had no sleep, and the diarrhœa was returning. He took some opium, and repeated the quinine in smaller doses. After a good sleep he awoke much better, and was

apparently going on well, when after some days I was sent for, in consequence of a congested state of the left lung, and slight cough. The apex of that lung was quite dull on percussion: no respiratory movement was observed on that side, and bronchophony was distinct, and much large moist crepitation, which made me fear the result, as his mother was the only one of a large family who had not died of consumption. A blister was applied under the clavicle, and he was ordered port wine and nutritious food; and I am happy to say that he is now quite recovered, and the lung perfectly sound. I do not attribute the congested lung to the quinine, but to the fever; but I should be wrong to omit it in making a report.

"I requested the opinion of Mr. Hine, who has the care of the servants of the great Western Railway at Swindon, including 2,000 families of artizans, etc., among whom typhus is prevalent,—perhaps from want of drainage in the new town,—and his report to me was, that for some time the result was most satisfactory; but latterly he had found that the head symptoms had prevented his using it. He thought that there had been some change in the type of the fever.

"The only drawback that I see to the use of the large doses of quinine, is the necessity for frequently visiting the patients, say several times a day, which is almost impossible in country practice, and very difficult in town when one is much occupied, and patients may consider such frequent visits to arise from the *nimia diligentia medici*, when the danger is over, and perhaps hardly apparent."

The above statement needs little comment. Of the eight hospital cases of typhus gravior, the treatment was conclusively satisfactory in seven; the exceptional case was also valuable, as proving the perfect safety of the remedy in those individuals where some peculiar condition or idiosyncrasy interferes with its curative power. In one case, Dr. Goolden pushed the remedy, not only with perfect safety, but with entire success, beyond what I have ever done, or found necessary; and this fact is very important.

The case of the private patient is highly instructive, as proving, beyond all rational doubt, the specific power of quinine in arresting, *within the short period of eight hours*, the most formidable and alarming symptoms incident to typhus fever: and it is especially important in another sense,—had the fever been prolonged, considering the patient's constitutional tendency, fatal disease of the lungs was inevitable. No other form of treatment, I firmly believe, could have saved this youth's life.

Though deficient in the necessary details, the report of Mr. Hine will fix attention; and I trust that this gentleman, as well as others, who enjoy such extensive and favourable opportunities for observation, will favour the profession with the results of their future experience. I must not pass over the "*only drawback*" urged by Dr. Goolden against the quinine treatment of typhus—and its practical difficulty is indisputable—namely, the necessity for frequently visiting the patient. To ensure success, he should, undoubtedly, be frequently seen, whilst the large doses are being administered; and this, as Dr. Goolden observes, is almost impossible in country practice, and very inconvenient to the well-employed town physician. This difficulty, however, affects in no degree the intrinsic value of the quinine system of practice, or the rationality and truth of the theory on which it is based. At the same time, it must be admitted, that the efficacy of this method of treatment cannot be fairly and fully tried, unless the practitioner, or some competent substitute, exercise a frequent, even though an inconvenient, supervision.

Neither in my own experience, nor in that of others, so far as I am aware, have the large doses of quinine caused congestion of any important organ. On the contrary, by cutting short, or moderating the febrile excitement, they prevent all such congestions, and in this consists the great value of the treatment; seeing that the vast majority of fever cases are carried off by these local affections. In the event of any important organ being involved, I have, with great advantage, resorted to extensive dry cupping, either alone, or followed by a blister.

When the first large doses of quinine have failed to produce their usual curative effect, and the practitioner is obliged to discontinue them, an emetic ought to be administered, as formerly pointed out; and if, after this, the febrile excitement still continue, a full dose of the liquor opii sedativus, with a few drops of nitric acid, will frequently afford the most signal relief, and enable the patient to resume the *quinine with* every prospect of success.

I would here observe, that the large doses of quinine are not only safe, but advantageous in every stage, and in every form of typhus fever, and that the action assigned to it by Dr. Pereira and others is altogether erroneous. The presence of intense headache, quick and strong pulse, dry and burning skin, dry, chapped and black tongue, intense thirst, hurried respiration, abdominal tenderness, and diarrhœa, do not contraindicate its use. On the contrary, under large and repeated doses, the headache will subside, the pulse calm down, the breathing become less frequent, perspiration will return, the tongue will become moist, and the diarrhœa will be checked.

After the first decided impression has been made on the disease by the quinine, it is invariably necessary to support the patient's strength by good beef-tea, and a moderate allowance of wine. Purgatives, without some decided necessity, should be avoided. When the head continues much involved, a strong capsicum enema—a drachm of the *powder* to ten ounces of water—will often afford relief. The minor adjuvants in fever may also occasionally be resorted to with comfort and advantage.

In these observations I have confined myself to the exposition of general principles, and altogether avoided cases, as I entirely concur with that eminent physician, Dr. William Stokes, whose 'Lectures on Fever' are unexcelled in the English or any other language, "that you might as well expect to find two human beings exactly alike, as to find two cases of fever perfectly similar";—the varieties are infinite.

I may here state, that I have repeatedly witnessed in Brazil, seasons of ague (for the disease prevails in seasons), when quinine, though always curative, appeared less efficient in controlling the disease than at others; and similar modifications will doubtless occasionally occur in the fever of this country. Modifications in the quinine treatment, and probably very important ones, will also, I am satisfied, be introduced through the more extended experience of the profession, though I believe the *principle* will be only confirmed by being tried and tested by different observers.

I shall now briefly repeat the principles I have here, and elsewhere, laid down. The evidence on which I adopted them is embodied in my 'Sketches of Brazil,' now in the press, and which in a short time will be laid before the profession.

1. Ague and remittent fever do not originate in malaria or marsh miasm. The doctrine of a special marsh poison I hope to show to be altogether unfounded.
2. Intermittent, remittent, and continued fever, are mere varieties of the same disease. The intermittent constantly emerges into the remittent or continued type; and continued fever assumes still more frequently (in Brazil) the intermittent form; and all are curable by the same agents. By the same agents we can arrest them all. Could this be done if they originated in different *specific* poisons? or, can we ever arrest, by any power, the course of a truly specific disease, as small pox, etc.?
3. The notion of typhus fever being unknown in the tropics, is altogether ill founded. The intermittent, remittent, and continued fevers of tropical climates often run into genuine typhus. In Brazil, when the disease takes this course, it is popularly termed "maligna," or "melina," and, in some seasons, is very frequent and very fatal.
4. Bark, duly administered, will generally arrest the intermittent and remittent fever; and typhus fever being essentially the same disease, bark ought to, and will, generally arrest it.
5. Ague will occasionally resist, for many days, the most judicious application of quinine—and finally yield: the same remark applies to typhus.
6. Quinine is more certain in its results in proportion to its early administration; but it is less to be depended on with the aged.
7. The administration of large doses of quinine in typhus, when not curative, is *never* followed by the slightest ill effects.
8. As typhus is, commonly, the more severe form of fever, and the subjects of its attack generally less favourably disposed, so we shall find considerable discrepancy in the several results.
9. Typhus will occasionally resist quinine, and yield to other remedies, and the

same holds good with ague; yet who ever associates the latter disease with any other remedy than quinine? And I am firmly of opinion, that the time approaches when the treatment of typhus fever, after ages of vacillation, will be established on the same sure and satisfactory basis as that on which the treatment of ague now rests.

[The following letter is in reply to one from Dr. Dundas, by Mr. A. B. STEELE, Medical Superintendent of Irish Quarantine and Fever Ships in the Mersey:—]

"The first case was an Irishman, named Lawrence Connor, aged 40, living in 4 Court, Grosvenor-street, one of the very worst streets in Liverpool, and scarcely ever free from fever. I visited him here on Saturday the 18th instant, and found him labouring under the usual symptoms of the low type of continued fever; had been ill seven days; was lying on straw in a corner of the floor, destitute of all comforts, or even necessaries.

"I gave an order for his removal to the Fever Hospital, but he did not go. On Monday the 20th, I was again called to him. He was now so much prostrated, that I should have considered it hazardous to remove him. There was great nervous and muscular debility; skin hot and dry; suffusion of conjunctivæ; pulse small and frequent; tongue protruded with difficulty, very dry and brown; delirious, especially at night; can scarcely answer questions. I ordered one drachm of disulphate of quinine in six powders, one to be taken every two hours.

"On the 21st I found the patient considerably better. The skin was moist; pulse softer, fuller, and less frequent; tongue readily protruded, moist and whitish; the brown fur had quite disappeared; delirium stated to be quite removed; expresses himself better. The quinine in ten-grain doses was ordered to be continued, and wine and beef tea to be given. These directions were not properly carried out during the following days, and a partial relapse has been the result; the tongue becoming rather dry and brown; the prostration returning; but still the patient is now (25th) in a far more favourable condition than he was anterior to the administration of the quinine.

"The second case, a woman named Galagher, 14, Collingwood-street, had been labouring under fever for several days. On the 20th, I found her in a state of great prostration, with well marked symptoms of low fever, with, I believe, pleuritic complication. The condition of the patient did not admit of auscultation, or a minute examination. General symptoms very similar to the first case. I ordered quinine in ten-grain doses, and a blister to the chest. The next day, she expressed herself much better; the symptoms had evidently given way. The quinine was continued, and she is now in a fair way of recovery.

"I have tried the plan in a third case of low continued fever, with dry brown tongue, etc.; but unfortunately, from the first, the medicine was not given regularly, or in the quantities ordered; still, what would be considered a large quantity of quinine was taken, and with a decidedly beneficial effect, although not to the same extent as in the other two cases.

"In the first case, the decided effect on the objective symptoms of the disease, *in twenty-four hours*, was so striking as at once to convince me of the value and importance of the remedy; and this improved condition was produced without the collateral advantages of *ventilation, cleanliness, nursing, nutritious diet, or stimulants*,—a fact which greatly enhances the value of this mode of treatment, in the hands of those who, unhappily, have to contend with the disease under the most unfavourable circumstances.

"I have witnessed the results of various methods of treating fever, in several hundreds of cases, during the epidemic of 1847, in our Fever Hospital, and on board the Fever Ships in the Mersey, and subsequently in the town; but I have never found any remedy or remedies, which appeared to me to cut short the disease, or modify the symptoms, in the same decided manner in which the quinine has done when fairly tried."

[The next communication is from Mr. EDDOWES, of the Liverpool Fever Hospital. Mr. E. says:]

"In one important practical point, I entirely differ from Dr. Goolden and yourself. *I do not consider* that the quinine treatment requires, for its successful

employment, any special supervision. For all practical purposes, *one* visit daily to the patient has been found sufficient in the hospital, where the ten-grain doses have been given every two hours, for many days, without any inconvenience.

"I quite agree with Dr. Goolden (judging from the cases under Dr. Gee, and in my own charge), that the treatment appears to be altogether free from danger.

"The quinine has been used here in fevers, complicated with chest affections, etc. I enclose for your satisfaction the notes of two cases (*ex multis aliis*), and should you desire any more you can have them.

"*Case* I. *Typhus in an Epileptic—Erysipelas supervening—Convalescence on the Fifth Day of Treatment.* Richard Lewis, aged 30, a painter, has had colic and wrist-drop; has had epilepsy for seven years, having two fits a month. The epilepsy occurred a month after the attack of colic. His general health is good.

"*Present Attack.* Has been ill more or less for three weeks, but confined to bed for five days only: he had been under treatment and had got worse.

October 18. *Present State.* He complains of deafness and frontal cephalalgia; is propped up in bed, which, he says, eases the great headache. He passes restless nights; the countenance is flushed; the respiration hurried, 32; the pulse 106, jerking and weak; skin hot and dry; tongue dry, and brown in the centre, moist at the edges; there is tenderness upon firm pressure over the hepatic region. He was directed to have ten grains of the disulphate of quinine every two hours; four ounces of brandy daily; milk diet, beef tea, and arrow-root.

"Oct. 19. He sleeps better; the head is easier than yesterday; is not propped up as before; the breathing is easier; the tongue as before; a dusky erysipelatous flush is appearing on the cheeks and forehead. He was ordered to continue the remedies, and apply flour to the erysipelas.

"Oct. 20. The headache is less, but he spent the night restlessly; the face is swollen; the breathing natural; pulse 96; tongue as before. He says that he 'feels quite well, except the soreness of the head and face.' To continue the quinine, etc.

"Oct. 21. The deafness is nearly gone; the tongue moist; he was restless at night; headache quite gone; pulse 80, natural; skin cool, covered with a perspiration; the erysipelas is better. The medicines were continued.

"Oct. 23.—He was convalescent; and was directed to take ten grains of quinine three times a day.

"*Case* II.—*Petechial Typhus—Pleuritis and Bronchitis—Convalescence on the Fifth Day of Treatment.* Oct. 23, Mary Maloney, aged 15, of good general health, has been ill five days; the skin is hot and dry, with petechiæ; she has great thirst; restless nights; slight headache, tongue coated with a white fur; loss of appetite. The pulse is 100, natural. ℞. Ipecac. g. xv., antimon. potass. tart. gr. i., statim sumendus. Postea sumat quinæ disulph. gr. v., secundis horis. To have milk diet, arrow-root, and beef-tea.

"Oct. 14. She is breathing quickly; the pulse is 120, jerking and weak; the tongue white at the base; she has pain in the left inferior mammary region. In front, the chest is clear on percussion; there is sibilus on the right side, also on the left, with occasional cooing rhonchi. To the left of the cardiac region is a dry friction sound, loudest during expiration. Behind the left base is resonant, but less so than the right; the respiratory murmur is faint throughout. The friction sound is audible from the supra-spinous fossa to the base; mucous rhonchi are audible over the whole of the right side. To continue the quina. ℞. Ung. hydrargyri fort. ʒij.; pulv. camphoræ 3ss.; p. opii 3j. M. Sæpe lateri sinistro infricandum.

"Oct. 15. The pain in the side is easier; the respirations lower; pulse 118; skin hot; friction sound behind as before; no increase of dulness; friction sound in front, moister. Continue the quina.

"Oct. 16. The breathing is easier; the pulse softer and more natural; the countenance improving.

"Oct. 17. Pulse 112, soft and natural; the skin moist; no cough; no headache; breathing natural; petechiæ fading; she takes food, and says her tongue is sore. The quinine was continued.

"Oct. 18. She is convalescent; the tongue is clear; the countenance natural;

pulse 84. She says she feels well. The friction sound is still audible. She had no relapse.

"There were two other patients in the same ward with the same complication: the above treatment was adopted, and the recovery was as speedy.

"Perhaps it may not be uninteresting to you to know, that there have been two cases of typhus occurring in pregnant women. The quinine was administered—in the one case five, and the other ten grains—every two hours. They both recovered without any ill effects."

I have lately conversed with several able men of great experience—Dr. Ewing Whittle of the South Dispensary, amongst others—who have observed fever on a large scale, and who are of opinion that no reliance can be placed on the "eruptions," to which many distinguished authors attach so much value, as diagnostic of the different fevers. They consider the character of the eruption as dependent on epidemic constitutions, idiosyncrasy, and atmospheric and other influences. All are aware of the numerous "rashes" which supervene on derangement of the digestive functions from the use of certain medicines, articles of diet, &c. in different constitutions, and at different seasons.

As regards the rosy lenticular rash, deemed peculiar to typhoid fever, all tropical practitioners must repeatedly have observed these spots in protracted cases of dysentery; and I have myself witnessed all the eruptions described by authors as *pathognomonic* of the several fevers, displayed in the same patient, at one period or other of his disease. There is now a patient in the Liverpool Fever Hospital, A. B., who presents an abundant "*mulberry rash*," which quite disappears on pressure.

There is also, at the present moment, another patient in the Hospital (James Moore), in whom we have conjoined the mulberry rash, the rosy lenticular rash, and true "erysipelas," the latter classed by Dr. Watson, one of the latest and best authorities, with the contagious exanthemata—as "a specific disease, running a definite course, and attended with an eruption."

Now, in this instance we have, according to authority, three distinct morbid poisons—the typhus, the typhoid, and the erysipelatous—contending for mastery in the same unhappy individual, and all running their regular course, unchecked and unmodified in the slightest degree. Does the history of other morbid poisons present us with anything analogous?

On the authority, indeed, of Carmichael, a "plurality of venereal poisons" was at one time pretty generally admitted by the profession; but was finally exploded by M. Ricord of Paris, who, on one occasion, exhibited, with a smile, to Mr. Carmichael himself, his *four* distinctive eruptions classically designed upon one and the same patient.

This *argumentum ad hominem* did not, I believe, prove altogether conclusive to Mr. C., but perfectly so to every one else; and the "plurality of venereal poisons" soon disappeared.

All must admit that our lot is cast in revolutionary times. I, however, as a loyal citizen of the republic of medicine, have now discharged my duty in handing over "the quinine system," and the principles on which it is based, for trial before the "legal and constituted authorities."

Like other arch-revolutionists, the present doctrine is earnest in its *promises* to "benefit the public." I have not, however, allowed its justification to rest solely on my own testimony to character—naturally open to challenge—but have adduced other, and unimpeachable evidence, and I now await with confidence the verdict; for although the profession be a republic, and its decrees too often tinged by human infirmity, I firmly believe that its final judgments are never wanting in calmness, and justice, and truth.

An incidental interest attaches to the doctrine now advocated, namely, that it will afford an opportunity for testing the value of the infinitesimal doses of homœopathy with doses even larger than those commonly employed by regular physicians. Let an adequate number of fever cases be selected, place them side by side in the same room; let six be treated on my plan; six infinitesimally; let competent individuals (not including myself) be appointed on either side to take the notes, day by day, administer the remedies, and report the results; and on these results I am *willing to stake, absolutely, my own* professional reputation, and the reputation of

legitimate medicine, so far as that can be staked by such an humble individual as myself. I entertain no doubt that the authorities of the Fever Hospital or of the Infirmary, the Northern or the Southern Hospitals, or the Dispensaries, will readily afford the means of testing by direct comparison, that which I believe, with the rest of the profession, to be a dangerous delusion, but which has obtained sufficient extension to render its refutation an object of public interest. To this "experimentum crucis" no honest homœopath can object.—*London Journal of Medicine, Dec.*, 1851, *p.* 1103.

2.—*On the Fevers of New Orleans, particularly the Yellow Fever of* 1849.—*Abortive Treatment by Quinine.* By DR. E. D. FENNER.—The following is derived from Dr. Fenner's valuable Report on the Fevers of New Orleans, originally published in 1849, in the 'Southern Medical Reports,' vol. i. p. 109; and reprinted in the 'Edinburgh Med. and Surg. Journal' for October, 1851.

In New Orleans, as well as in the surrounding country, the fevers are intermittent, remittent, and continued, "alternating in type, and running into each other." In summer and autumn, they have a decided tendency to crisis by hemorrhage: "this," says Dr. Fenner, "*makes yellow fever:* it forms the true characteristic difference between the high degrees of summer and autumnal fever in the city and country, and must depend on locality and attendant circumstances. During the healthiest years, it predominates over all other types; but during the sicklier years, in the country, it runs into remittent, bilious, and congestive; whilst in the city, it runs into yellow fever. Dr. Harrison testifies, that he had often observed malignant intermittents immediately to precede the outbreaks of yellow fever epidemics."

Dr. Fenner's observations being drawn from a vast public and private experience, and in particular from observations made in, and from the statistics of, the New Orleans Charity Hospital, which is probably the most extensive fever hospital in the world, the following statement is of peculiar force:—"Physicians may say what they please about being able to distinguish a case of yellow fever, as soon as they examine it; we do not believe it possible, according to their ideas. Rarely does a summer pass, in which we do not hear of some intelligent aud experienced practitioner being perfectly astonished at seeing what he had pronounced a case of intermittent or remittent bilious fever terminate in black vomit or other hemorrhage."

Dr. Fenner is a very warm advocate in favour of the administration of large doses of quinine for the purpose of cutting short yellow fever. In fact, he urges the excellence of the abortive treatment of yellow fever, with the same confidence that Dr. Dundas recommends the same treatment in the continued fevers of this country. The experience of Dr. Fenner certainly gives additional strength to the important practical doctrine, which Dr. Dundas has brought before the profession.

The following are Dr. Fenner's remarks on the therapeutic action of the sulphate of quinine:—"The sedative powers of large doses of quinine, given during the early exacerbation of our summer and autumnal fevers, remittent, bilious, and yellow, have been proclaimed by some of the physicians of this city, by the army surgeons, and by the physicians of the Southern States, in the medical journals for the last ten years. We perceive that these views are gradually extending to the North, and will certainly take there, if properly tested; but it will require some time to prepare the minds of our northern brethren for such a revolution in therapeutics, as they must effect when established. The wonderful powers of the sulphate of quinine over the yellow fever of New Orleans, are briefly but forcibly set forth in the essay of the late Professor John Harrison, in the second volume of the New Orleans 'Medical and Surgical Journal.' Also by Dr. J Beugnot, a prominent French practitioner of your city, in the first volume of the same journal. Dr. Beugnot informs us, in a recent conversation, that notwithstanding the favourable mention he then made of the abortive treatment, by large doses of quinine immediately after free blood-letting, he did not adopt it; but relied chiefly on the lancet alone." . . . . . "Quinine is given in five or ten grain doses by a number of physicians of this city, and the southern country, who have not yet adopted the

abortive method. This is a great improvement; but they will go farther after a while. We saw twenty grains with twenty-five or thirty drops of laudanum given at the Charity Hospital, in the early stages of yellow fever, by physicians, this season, who would have shuddered at such a prescription three years ago."—*London Journal of Med., Dec.*, 1851, *p.* 1181.

3.—*On the Administration of a Single Dose of Quinine in Intermittent Fever.* By Dr. Meyer.—In a former number of this journal, we quoted what seemed a somewhat extraordinary statement by Dr. Pfeufer, that he had been very successful in treating ague by the administration of a single ten-grain dose of quinine, allowing at the same time a generous diet. In the present paper, Dr. Meyer states the results of a trial he made of this plan upon some of the military at Berlin. From five to ten hours prior to the expected paroxysm, ten grains of quinine were given with ten grains of sugar, a nutritious diet of meat and beer afterwards ordered, and ferruginous preparations administered. The cases amounted to twenty-eight, three tertians and twenty-five quartans, the disease having continued from two weeks to eighteen months, and for the most part having been treated without success, or at all events without durable success, by quinine, arsenic, &c. A tabular view of the results is given, the cases being ranged in three categories. In the first, consisting of ten patients, the single dose, followed by good diet and martial preparations, though it suspended the attack for a period varying from eight to twenty-eight days, did not cure the disease. These patients exhibited œdematous countenances, cachectic habit, and considerable enlargement of the liver and spleen. The second category of sixteen patients (three tertians and thirteen quartans, the majority of three or four months' duration) were cured by the single dose; and the two cases of the other category were cured by a repetition of the ten-grain dose after a fortnight. The conclusion Dr. Meyer arrives at is, that not only will the majority of simple intermittents yield to the ten-grain dose, followed by good diet and *ammon. mur. ferrugin.*, but that this treatment suffices in most cases of obstinate quartan, complicated with enlarged spleen and liver, when endemic influences do not maintain the disease, or have not induced such changes in the blood as to require more time to overcome.—*Casper's Wochenschrift*, 1851, No. 27.—*Brit. and For. Medico-Chirurg. Review, Jan.*, 1852, *p.* 275.

4.—ON FEVER.

By Dr. R. B. Todd, F.R.S., &c.

[Dr. Todd fully agrees in the doctrine, that though we cannot cure a fever, yet we may guide it through its several stages, and by careful management prevent the patient dying from exhaustion. He also believes the same with regard to the exanthemata. Many have boasted the power of cutting short these attacks of fever, but Dr. Todd believes that there has been some error in diagnosing the disease itself, and remarks how difficult, nay, how impossible it often is, in the first week or ten days to predicate with this or that case that it is typhus fever. The subject of the case related was a man named John Gavin, 32 years of age, a large bony man of strong build.]

His illness probably commenced in Edinburgh, and developed itself immediately on his arrival in London. It is often extremely difficult to fix precisely the day on which a fever began, partly from the imperfect recollections of patients, and partly because the symptoms often develop themselves so insidiously and gradually, that the patient cannot note exactly the time when he really began to be ill; he feels for many days languid and out of sorts, but is still able to get about, and, unless some such prominent symptom as rigor has occurred, it is impossible to name one day more than another on which the fever began. Now, what we gather is this,—that on or about the 9th, as he was leaving Edinburgh, he caught cold, of which he has no other evidence than the existence of great languor and weakness, with a strong sense of fatigue upon the slightest exertion. On his arrival in London,

he found himself quite unequal to the task of looking out for work, and unable to follow his business if he had succeeded in securing employment.

All this looks very much as if he had caught the infection in Edinburgh, where, we know, fever is always more or less rife among the lower orders; its period of incubation being the day or two before he left that city, and the first few days after his arrival in London. During the first week of his arrival in town the sense of languor increased, and he felt very ill. On the 16th of January, 1851, sore throat came on, and he was attacked with several severe rigors, succeeded, on the 17th, by increased debility, vomiting, head-ache, and tinnitus aurium. On the 18th these symptoms had increased in severity, and his friends stated that he became stupid, and appeared as if drunk, and at times he wandered a little. It was, then, on the 16th that the more decided symptoms of fever had developed themselves, although we cannot doubt that the poison had already begun to work in his system at least seven or eight days before that date.

He was admitted into the hospital on the evening of the 19th of January. On the 20th, the following report of his condition that day was entered in the case-book:—"The patient is very thin and weak; has a dull, vacant look; is delirious, incoherent, and it is not without great difficulty that answers to questions can be extracted from him; he is, however, very quiet, and lies chiefly on his back; respiration hurried; crepitation audible all over the posterior surface of both lungs, especially at their bases; the tongue is dry, but not coated; slight sordes on the lips and teeth; the abdomen slightly prominent and tympanitic; has had one loose motion in the night; no spots are observable; pulse 130, very compressible; respirations 44. He was ordered six ounces of brandy and a pint of beef tea daily, and five grains of the sesqui-carbonate of ammonia, with half a drachm of chloric ether, in an ounce and a half of water, every six hours, and turpentine stupes to be applied freely to the back."

On the 21st his symptoms had not changed, and the pulse was 128; respirations 40.

This description portrays very accurately the condition and the symptoms of a patient labouring under the most common form of continued fever now met with in London and our other great towns. On the 21st, reckoning from the date of the occurrence of rigors, the 16th must have been, at the earliest, in the fifth day of the fever,—it might be the seventh or eighth. This form of fever, which some French and American physicians, and more recently, some in our own country, seek to distinguish from typhus by the name *typhoid*, or by what I think, in some degree, a preferable name, *enteric*, is especially characterized by a remarkable tendency to a congested state of the bronchial passages and lungs,—a symptom which was well marked in our patient; and also by a strong disposition to irritation and inflammation, and subsequent ulceration of Peyer's patches, and even of the solitary glands in the small intestine. On account of this latter symptom, it is designated by German physicians, *typhus abdominalis*.

There is yet another sign in this form of fever very common, although, perhaps, not quite constant, namely, the occurrence of peculiar spots or *maculæ*,—circular, rose-coloured spots, varying in diameter from a line to a line and a half, sometimes existing so thickly all over the body, that I have, at the first glance, fancied I have had a case of measles before me, and sometimes so few in number, that they can with difficulty be detected. They generally occur on the abdomen, chest, and back; rarely upon the extremities, and seldom if ever on the face. These spots are very slightly elevated, and disappear under the pressure of the finger. They do not generally occur before the sixth day; but, in our patient, they did not appear throughout his illness, or, if they did, they lasted but a short time, and so escaped our notice. He had, what is very common in these patients, a dusky hue of skin, with here and there minute specks, looking like half-finished flea-bites—a state evidently denoting feebleness of the capillary circulation of the skin. It is very possible that such a macula may have appeared and disappeared without attracting notice. They come in successive crops, each of which lasts but a short time, to be followed by another crop equally evanescent.

In a former lecture, in which I had occasion to refer to this subject, I pointed out to you the distinction between these maculæ and petechiæ; the former being a true eruption, the latter minute spots of extravasation. The former character-

istic of this form of faver; the latter common to it, with other fevers and diseases of debility.

[From the 5th day the symptoms continued the same until the 17th or 18th, or to the 21st, or even 28th day: this is the same in general fever. The most important symptoms being those referrible to the nervous system—coma, or delirium; to the lungs—congestion, or even pneumonia or pleurisy, which are less frequent; and to the bowels, diarrhœa.]

When a case is about to terminate favourably, these symptoms gradually give way; the pulse exhibits no tendency to quicken, but rather to fall in frequency; the bowel affection appears easily controllable; the tongue begins to clear at the tip and edges; the patient becomes less stupid; the comatose or delirious state diminishes; the pulse improves in quality, and the general powers of the patient experience a gradual but manifest change for the better. These changes commence generally in or about the third week.

But, if the case is not about to end favourably, we shall find an aggravation of some of these symptoms about this period. The pulse will increase in frequency, and its power will be much diminished; the delirium and other head symptoms will become more alarming, or the symptoms referrible to the lungs may become more severe, the breathing more rapid and feeble; the bronchial tubes may become plugged up with mucus, which the patient has not sufficient power to expel, and, in consequence, death may result from asphyxia; or he may be run down by the constancy or profuseness of the diarrhœa, and perhaps by hemorrhage from the bowels.

Now let us see what was the further course of the symptoms in John Gavin's case.

On the 23rd of January he had in some degree recovered the exhaustion caused by his removal to the hospital. His pulse had fallen to 112, but the respirations continued at 48. He was purged four times in the day; the chest signs remained the same.

An enema of starch and opium was ordered at night to counteract the diarrhœa, and his brandy, ammonia, and beef-tea were continued as before. The motions became less frequent, and he remained without any change till the 27th.

On this day we found the bowels with a tendency to be loose again; three motions in the day; abdomen tympanitic; pulse 120; and respirations 52. Many of you will remember that I pointed out to you on this occasion a good mode of estimating the real power of the pulse in fever, and other asthenic states, namely, by causing the patient to sit up in bed, and comparing the condition of the pulse in this semi-erect posture with its state in the horizontal position. It was not accelerated by the change from the horizontal position, but its strength and volume became most strikingly diminished; it became very small, and much more compressible, but immediately he resumed the horizontal position, it recovered itself.

There cannot, I apprehend, be a more palpable or unequivocal sign of an enfeebled circulation than this marked deterioration in the quality of the pulse on the patient's assuming the semi-erect from the horizontal posture. It indicates very clearly how dangerous it is to move patients in fever or other low diseases, or to allow them to move themselves, and how necessary for them it is that they should be constantly attended upon, that every, even the slightest, exertion on their parts should be prevented as much as possible.

It was now evident, that what we had chiefly to deal with was the extreme debility and looseness of the bowels. This state of debility was the more fearful, inasmuch as it had come on, notwithstanding the free use of stimulants, since the 21st, for during that time he had been taking brandy at the rate of half an ounce every hour. I now doubled the quantity of brandy, and ordered the ammonia and chloric ether to be taken in an ounce and a half of infusion of rhatany every fourth hour.

[On the 28th and 29th, although the rhonchus in the chest had increased, the general debility had not diminished, and the stimulants were increased to five drachms every half hour, or 30 ounces in the day. On the 31st the rhonchus was less, and the pulse had fallen to 112; respirations 46. On the 1st of February the

pulse was down to 100; respirations 45. On the 3d the pulse was only 84, and the respirations 38. All his symptoms were now considerably improved: the least improvement was found in his consciousness. In consequence of this stupor continuing, the quantity of stimulants was reduced by 6 ounces per day, so that he now took ℥j. every hour, instead of 3x. The chloric ether was also omitted. From this time all his symptoms became gradually but rapidly worse. On the 7th a quantity of blood passed by the bowels, still further exhausting him, and he died on the morning of the 9th, which must have been the thirteenth day of the fever.

Now, it behoves us to inquire, why did this patient die? Was there here the *nimia medici diligentia?* Were the quantities of food or stimulants too much for him? Was there any other treatment which he did not use, but which we ought to have had recourse to? Or did death result from causes clearly beyond the control of all medical interference?

The *post-mortem* inspection showed that the morbid changes were limited to the chest and abdomen. In the former there was congestion of the lungs; but to an extent decidedly less than we had expected. The bronchial tubes, however, contained a considerable quantity of the thick yellow purulent matter which he was expectorating during the last few days of life.

But the most serious lesion was in the intestines. The lower part of the ileum contained numerous deep ulcers, some of which had eaten through the coats of the intestines so as almost to perforate. These ulcers were placed on the free margin of the intestine, and occupied the position of Peyer's patches. In the lower three feet of the ileum, we counted as many as seventeen ulcers; some of them were larger than a shilling. The floors of some of them consisted only of peritoneum and a little lymph. One very large ulcer existed on the iliac side of the ilio-cœcal valve. In addition, several of the solitary glands were enlarged, and some ulcerated, and the mesenteric glands were enlarged.

I need hardly say, that from our experience of cases of this kind, and from the diarrhœa, controllable although it was, and the tympanitis, and the hemorrhage ultimately, we were quite prepared to find ulcerated disease in the intestine; although, owing to the mildness of the symptoms referrible to the bowels, we might well be surprised to find such large ulcers, and so many of them.

This extensive lesion of the mucous membrane of a part of the intestinal canal so important to nutrition as the ileum, must have contributed mainly to the state of prostration of this patient which persisted for so long a time, notwithstanding the abundant supplies of nourishment which were given him. And yet it is difficult to explain precisely how these ulcerations could have occasioned all this debility, inasmuch as there was no excessive diarrhœa, no great drain from his system, nor did they interfere with the due digestion and absorption of his food, for the quantity of fæces formed was not unusually great, nor out of proportion to the amount of food taken. It is plain enough, that notwithstanding the disease in the ileum, gastric and duodenal digestion and chylous absorption in the jejunum must have gone sufficiently to admit of the appropriation of the greatest part of the food given.

But the ulcers are not the only mischief existing in connexion with the bowels in these cases. The mesenteric glands are likewise diseased, swollen, and evidently irritated by some abnormal matter passing through them. No doubt the state of these interferes with due chylous absorption, but still scarcely to a sufficient extent to account for the prostration, for the food is absorbed, and a good deal of it is of a nature (as the oily matter of the milk), which must assume the state of chyle prior to absorption.

It seems to me that the production of this state is due not so much to imperfect appropriation of food, as to the absorption of a matter from the ulcerated surfaces, which, circulating with the blood, exercises a poisonous and depressing influence—a matter of the nature of, if not identical with, pus—which is absorbed by the lacteals, and perhaps also by the blood-vessels, but probably chiefly by the former, by which route it quickly reaches the lungs, without passing through the liver, where it may contribute to the increase of the bronchial congestion and irritation which so constantly accompany this typhoid state. This view I have often broached to you already at the *bed-side of* patients suffering in this way.

I show you here a preparation which was put up for me some time ago by Dr. Beale. It exhibits a few well-marked deeply perforating ulcers of the ileum, having much the appearance, from the thick, swollen, and red margins, that the process of sloughing and ulceration was a quick one. In this case (the patient was a young woman), the fever ran its course in fourteen days, the diarrhœa was almost gone, and the chief symptoms were a tympanitic abdomen, stupor (in fact coma), bronchial congestion, and extreme prostration. A short time ago, you may remember, a woman of the name of Lock, who went off very quickly likewise with similar symptoms, the stupor being so great that I was afraid that a few drops of laudanum administered with starch, to check diarrhœa, had narcotised her. There was in this case, in addition to the stupor, bronchial congestion and prostration, but the diarrhœa was very slight, and readily controllable.

Now, that the absorption of pus is capable of producing these depressing effects we have many proofs.

First, in puerperal fever. In some cases the absorption seems to take place rapidly, and in large quantity, and, under such circumstances, the patient succumbs in a few hours from rapid prostration and pulmonary congestion, with more or less stupor. In other cases, the absorption seems more gradual, the typhoid condition is induced more slowly but very completely, and, after a time, purulent deposits are found in the joints and muscles.

Secondly, in cases of erysipelas, in which the suppurative process is rapid, we have typhoid and comatose symptoms, which are out of proportion to the extent of lesion; in such cases doubtless pus finds admission into the circulation.

Thirdly. We sometimes have unequivocal evidence of the absorption of pus, as well as to the source whence it comes, as with respect to the secondary deposits. I remember attending a case in private practice, where the pus showed itself in the anterior chamber of the eye. This case presented all the symptoms of typhus fever; and for a day or two I viewed it as such. I was one day much surprised at observing pus in the anterior chamber, which increased in quantity very rapidly, and pus was afterwards found in the elbow and shoulder joints. When we came to examine this patient, we found an ulcer in the heart, at the base of one of the mitral valves. Some years ago, we had a case in the hospital, of a woman who was suffering from chronic bronchitis; she suddenly became typhoid, and I looked upon it as a case of most aggravated character. She died in a few days, and we found an abscess in the septum of the heart, which had burst, and thus the pus had entered the very fountain of the circulation, producing the symptoms exactly resembling those which come on in a case of low typhoid fever.

There seem, then, sufficient grounds for explaining the prostration and fatal termination in Gavin's case, without ascribing any ill effects to either what had been done for him, or to what had been left undone. The sloughing and ulcerative process undoubtedly interferes, to a certain extent, with the function of the bowel, but it also furnishes a source of formation of a poisonous matter, which, we know by experience of analogous cases, when taken into the system, creates symptoms of the same character in other cases.

There is another mode of termination of these cases of typhoid or enteric fever for which you should yourselves be prepared, and for which you should prepare the friends of the patient when you may see sufficient reason to apprehend it. I mean, that by perforation, one of those films of peritoneum, which I have already alluded to as forming the floor of many of the ulcers, gives way, and the contents of the bowel pass into the peritoneal sac. In some cases of long duration, when the patient seems to have struggled, day after day, against the assaults of death, rapid sinking immediately follows the perforation, and, indeed, signalises its occurrence. No new pain is felt, but the patient grows rapidly weaker; the pulse, too, fails, becomes rapid and fluttering, and death from exhaustion or fainting quickly ensues. In other cases the occurrence of the perforation ushers in severe pain in the abdomen; sometimes vomiting; tenderness and pain on pressure; tympanitis; with also increased prostration; all signs of peritonitis induced by the irritating influence of the intestinal contents upon the peritoneum. When these latter symptoms make their appearance, the free exhibition of opium in large and frequently repeated doses is the only measure to which the practitioner can have recourse with any *hope* of success.

Had Gavin not been carried off by the exhaustion consequent on the hemorrhage, it is very probable, from the state of the ulcers, that perforation must have taken place, of which he would have died in either of the two ways which I have described.

A third mode of termination is by colliquative diarrhœa. The patient may be going on well, and the practitioner may even be sanguine in his expectation of a favourable result, when the diarrhœa may suddenly become colliquative, and a few discharges of large watery evacuations will carry off the patient.

But to return to the treatment of this case. It may be said, surely the irritation of the bowels was kept up by all the stimulants (to say nothing of the food) which were given, and had they been more sparingly given the ulcerative process in the ileum would not have gone so far.

This notion respecting the injurious effects of alcoholic stimulants, in cases where there is a tendency to bowel affection, is, I think, partly founded upon a vague supposition that the alcoholic fluid comes in direct contact with the irritable mucous membrane; whereas we have the strongest reason to conclude, that fluids of this kind never pass the pylorus, but are absorbed by the walls of the stomach. This is especially the case when they are administered in the way I recommend,—that in which they were given in Gavin's case,—namely, in small quantities, with intervals of not less than half an hour between each dose. Thus one dose is absorbed before the other is given.

But it may be urged, that the alcohol gets into the blood, circulates with it, and so increases the tendency to ulceration.

Now, upon this point we can only appeal to experience. The administration of alcohol to healthy persons does not prove injurious by any irritative effects it may produce on the bowels. Of all the ill consequences which the advocates of the teetotal system, in their most praiseworthy zeal, have summed up as likely to be caused by the use of alcohol, I do not find that diarrhœa or ulceration of the bowels is noticed; and were it a frequent effect, it certainly would not have escaped the scrutiny of these gentlemen. It is true that a debauch, in which a man may drink at one sitting as much, or considerably more, than we should think of giving in twenty-four hours, may sometimes disturb the liver, and, through its increased secretion of bile, the bowels; but the looseness thus excited seldom or never proves otherwise than salutary.

Nor do we find that effects of this kind are apt to follow the liberal administration of alcoholic stimulants in other low diseases; in erysipelas; in the diffuse inflammation of the areolar tissue, whether traumatic or not; in puerperal cases; and we give it repeatedly in cases with threatened or actual ulceration of the bowels, without any increase, but, on the contrary, a marked diminution of the unfavourable symptoms. Such, indeed, was the case with our patient Gavin. On the first few days of his taking stimulants, a manifest improvement took place in all his symptoms, those affecting the bowels as well; so much so, that until the *post-mortem* examination revealed the true state of matters, I blamed myself for diminishing his supply of stimulants on the 3rd. Probably the good effects continued until the purulent matter had entered the circulation in sufficient quantity to produce its poisonous effects.

I have made these remarks to you upon the subject of the treatment of fever by stimulants (and they apply not less to the treatment of other exhausting diseases—erysipelas, influenza, bronchitis, carbuncle, &c.), because I wish to caution you against the morbid fear of over-stimulation, which leads many to adopt a contrary course, and to allow their patients to die from exhaustion. This is the mode of death to which typhus patients are peculiarly prone; and I hold that the lower you allow them to become at first, the more likely is the ulcerative process in the intestines to take head just as it is apt to do in the bowels and in the cornea of the eyes, in cases where there is an insufficient supply of properly nutritious food. At the same time, I must beg that you will not run away with the notion, that every case of fever about which you may be consulted, is to be treated with thirty ounces of brandy a day. There are many cases in which no stimulant at all is necessary; others, again, in which it is never needful to give more than four or six ounces a day. You must bear in mind that we have two classes of cases of fever to deal with, the mild and severe, or those which have had a large and those which have

had a small dose of the peculiar poison on which the febrile state depends. Where a large dose of the poison has been received into the system, you will generally find it necessary to give large quantities of stimulants, or the patient will not have sufficient vital power to resist the depressing effects of the poison. Some few cases, indeed, there are in which the dose of the poison is so large, that the patient never rallies from the state of almost complete paralysis which it induces; such cases run their course in twenty-four or forty-eight hours, or within a week. But the mild cases—and fortunately in many epidemics these are the most numerous—do perfectly well on a very moderate amount of nourishment, with little or no stimulant.

The objections which some excellent practitioners have to the use of stimulants apply with more justice to the slovenly mode in which they are too often given. Frequently left altogether to the discretion of a nurse, or given in large doses at once, or with other food, or without any reference to the medicines which are being likewise administered, they create derangement of the primary or stomach digestion, flatulence, and flushing. If you give stimulants, give them with due regard to their digestion by the stomach, and so as not to interfere with the other food, or the medicines likewise being given.

I am convinced that it is much better to err on the side of over-stimulation than not to give enough; for, if we have over-stimulated a patient, it is very easy to pull him down again, there are plenty of appliances and means to effect this purpose; but if the patient sink too low, nothing is more difficult than to restore him. If by your feeding and stimulating, the thermometer of life has risen to too high a point, nothing is easier than to depress it; but if fallen below a certain point, then to raise it again, much more to restore it to the height from which it fell, "*hic labor, hoc opus est.*"

In conclusion, let me say a word or two as to the treatment to be pursued when you have reason to fear that the bowels are ulcerated. It seems to me that the great principle of treatment in such cases is to keep down peristaltic action, which is best done by opium and astringents containing tannin or gallic acid. Many attach great value to the use of sulphate of copper; but, as it is generally given with opium, and certainly does not agree without opium, I think the latter drug has the largest claim to the good services often done by the combination. When hemorrhage occurs, nothing is so effectual to restrain it as turpentine given in small doses, so as not to risk offending the stomach; even so small a dose as five minims is frequently sufficient; and I often apply it externally as a stupe to the walls of the abdomen with decided benefit. In dealing with these cases, you must not be timid as to allowing the bowels to remain inactive for even several days. I have never seen any bad consequence from their not acting even four or six days; and when they are to be provoked to act, let that be done by some simple enema rather than by aperient medicine.—*Med. Times and Gazette, Feb.* 21, 1852, *p.* 179.

[In another clinical lecture, Dr. Todd further remarks on the treatment of these cases.]

One important feature of fever, whether it be typhus or typhoid, whether diarrhœa be present or not, is *depression.* The disease is adynamic, and great attention must therefore be paid to supplying the patient with a proper nutriment. The basis of his diet should be proteinaceous matters, in such a state that the stomach shall have little or nothing to do to bring them to a condition fit for absorption. In the animal broths, well made, and in milk, you have food which answers to this description. The former, on the whole, are probably the best. Milk is less easily digested, and does not always harmonize with other matters necessary to be given. Farinaceous matters may be introduced also in small quantities. A great secret of success in administering support to patients under these circumstances is this—to give it very frequently *in small quantities*, quantities so small that the whole or greater part of one supply may be absorbed before the next supply is brought; and also not to give a variety of food. Keep to milk and beef-tea, or other broth, or to broth or farinaceous matter.

In the great majority of cases you must, I think, give stimulants, and give them early. They often fail because begun too late. The best stimulants are brandy *and port* wine, with either of which chloric ether will go as well as a medicinal

stimulant; any one of the three will often suffice alone. Brandy and port wine ought not to be given together, simply because in general the stomach does not digest well two kinds of stimulants. The same rule as to frequent administration and in small quantities, which I have already laid down for food, holds with equal if not greater force in giving stimulants.

In my opinion, the question in the treatment of fever, is not whether you shall give stimulants, but how much you shall give. In many you may give as much as half an ounce every half hour, or even an ounce of brandy with advantage; but this is in bad cases. On this point you must be guided by the rapidity and compressibility of the pulse, and by the intensity of the heart's action. An important character of the pulse is found in the manner in which it strikes the finger; if *vacillating*, it is a decided indication for the use of stimulants. The strength of the heart's action, especially of the second sound, is also a good indication. If either sound be weak, but especially the second, you need not fear to give stimulants freely. An impulsive character of heart's action with a feeble sound, also denotes the use of stimulants. Under such a plan of treatment, in which nutritious fluids and stimulants are given freely and from an early period, we find our mortality in fever to be small; we very seldom lose a case of fever. I do not allow myself to be deterred from giving stimulants by the state of the bowels; I know that many have a fear that much alcoholic stimulant irritates the bowels. If the alcohol be given in small quantities each time, it cannot irritate it by direct contact, because it is absorbed before it reaches the intestines. Alcoholic stimulants, if not given too much at a time, are digested in the stomach, and the alcohol gets immediately absorbed and carried into the circulation. If it does harm, it does so from being in the blood; yet, I confess, I have never seen satisfactory evidence of this.

We must also pay close attention to the bowels. If diarrhœa be present, it must be checked by those astringents which contain tannin; as the infusion or tincture of rhatany, of catechu, of matico, of logwood, or you may give enemata with small quantities of laudanum. I find chalk often fails, and moreover it is liable to this objection, that as it does not dissolve, its particles may add to the irritation of the blood, by sticking in the ulcerated or inflamed patches. Counter-irritation over the abdomen by mustard, turpentine, or blister, is also frequently of great use. If there is hemorrhage, you may give small doses of turpentine, five minims repeated every three or four hours, and in such cases turpentine must be used as an external counter-irritant to the belly.

Another feature in these cases is the frequent occurrence of bronchitis or bronchial congestion, indicated by rhonchus and crepitation. The bronchial congestion and diarrhœa are frequently the most difficult symptoms we have to deal with in those cases in which we find maculæ. The bronchitis may be relieved by the free application of turpentine stupes, or blisters, to different parts of the chest at the same time or in succession; and though in such cases we must carefully watch the effects of our stimulants, we must not think of lowering our patient by bleeding, or by the administration of any antiphlogistic remedies.—*Med. Gazette, Dec.* 19, 1851, *p.* 1050.

5.—*Peculiar Appearance of the Tongue in Malarial Diseases.* By Dr. Osborne.—Dr. Osborne remarks that his attention has long been directed to a peculiar condition in the tongue of patients labouring under miasmatic diseases. It is an essential departure from the normal aspect of the edge, constituting a distinct lateral boundary of the tongue occupying more or less surface according to the charge of infection in the system. Ordinarily the colour amounts only to a very faint bluish tinge, which is liable to be lost or merged in the various tints imparted to the tongue by various diseases. The most fixed condition of this symptom is an appearance of indentation or crimpling transversely, which is apparently confined to the subjacent tissue,—while the superficial tegment is moist, smooth, and transparent. In a word, it seems to be a continuation or encroachment of the inferior surface upon the superior and lateral borders of the tongue, greater as we approach the root of that organ. The author states that the fidelity of this symptom to the source of its origin is, under all circumstances, fully equal

to the importance he has attached to it. In a considerable number of cases of the different forms of dropsy, neuralgia, and inflammation, it has alone enabled him to reach a correct diagnosis when every symptom seemed to deny the agency of malaria in the case. Wherever seen, he has invariably assumed that there existed a tendency to intermittent disease; and upon watching the progress of the case, has invariably detected this condition. In numerous cases of pulmonary inflammation, where the fever seemed continued, the cough unabated, the oppressive respiration persistent, and the pulse unvarying in its activity, the physical signs have designated with great accuracy the periods of repose and excitement in the course of the disease. In the gastric and intestinal phlogoses again, whether as causes or consequences of fever, if this peculiar impression is seen upon the tongue, he boldly states that no apprehension need be felt in the administration of quinine, however malignant the case may appear. Here also, intermission, varying in degree according to the severity of the disease, is always present, and, generally speaking, the shorter the period of repose the greater the quantity of medicine required; and *vice versa*. He has not hesitated, in many cases where the disease was marching rapidly to a fatal termination, to prescribe one, two, and even three drachms of sulphate of quinine at a dose, to be repeated according to the urgency of the indications.—'Western Journal, U. S., and Prov. Journal.'—*Monthly Journal of Med. Science, March*, 1852, *p.* 254.

6.—CASES OF RHEUMATISM CURED BY LEMON-JUICE.

(Under the care of Dr. Babington.)

[We have several times lately noticed the treatment of acute rheumatism by means of lemon-juice. The merit of introducing this remedy belongs to Dr. Owen Rees, and so high an opinion has Dr. Babington of its efficacy, that he mentioned it in his introductory lecture of the session, as occupying a prominent position among the strides which have been made in the various departments of medical knowledge since his pupilage. The following cases may be taken as good examples of its efficacy:]

The lemon-juice which was employed was procured from a wholesale confectioner, the price being no more than sixpence per pint, when retailed in small quantity, and considerably less when furnished by the gallon. When Dr. Babington commenced making trials of the lemon-juice, he prescribed it, according to the recommendation of Dr. Owen Rees, in doses of from one to two ounces, three times a day, for an adult. More recently, however, he had ordered not less than three ounces, and much more—usually six ounces—taken at a draught, and without any admixture, three times a day. It might be supposed that the patient would find some difficulty in drinking so large a quantity of such a sour liquid, but this is rarely the case; nor does it in general produce tormina, nor otherwise disagree with the alimentary canal. Instead of relaxing the bowels, as might have been expected, it renders them somewhat costive, so that it not unfrequently becomes necessary to exhibit an aperient. The juice produces no very decided effect upon the kidneys, merely tending by its quantity to promote their secretion. It increases cutaneous action, but to what extent it is difficult to determine, because in rheumatism, a disease in which this remedy is chiefly useful, the complaint itself is marked by the occurrence of profuse perspiration. The only unequivocal effect which uniformly takes place is a diminution in the number and power of the pulse, and of the heart's action. Whether it alters the character of the blood, or whether it affects the heart by diminishing nervous influence, Dr. Babington has not had an opportunity of determining.

We shall now shortly allude to eight cases, which were treated with lemon-juice by Dr. Babington.

Case I.—James D——, a tall and athletic young man, a dock labourer, twenty-two years of age, was admitted into Naaman's ward, August 20, 1851, with acute rheumatism. He has a white and coated tongue, profuse perspiration, thirst, a hard *full pulse at* 92, and costive bowels. There are swellings in the knees and wrists,

which are red and tender to the touch, and his debility is so great that he is scarcely able to walk to his bed.

The patient stated, on admission, that he had been ill three weeks, and that he had suffered from a similar attack eight years before, which lasted for one month. There were no irregular sounds about the heart.

Treatment.—To take six ounces of lemon-juice three times a day; a drachm of compound rhubarb powder to be given on the morning following admission.

First day.—Bowels relieved; pulse 76, softer; patient has less pain in the joints; to continue the remedy.

Third day.—Pulse 70, soft; no pain or swelling of the joints. Patient is up and dressed; he declares himself quite well, and requests to be discharged. He is asked to remain a day or two longer to improve his general strength; but he declines, as he states that he is well able to work.

Case II.—Samuel H——, aged 19, of slight form, a warehouseman, much exposed in his work to currents of air; never before had any similar attack. For four or five days he has had slight swelling and tenderness in the knees and ankles.

On admission into Naaman's ward, on September 10, 1851, there were great swelling, redness, and intense pain of the right elbow-joint, extending both upwards for several inches, and downwards into the wrist. The tumefaction was so severe, that it was a question whether the inflammation was not rather phlegmonous than rheumatic, but the pain and swelling in the other joints settled this point. Tongue moist but white; pulse 116, full; bowels regular; skin perspirable. To take six ounces of lemon-juice three times a day; low diet.

First day.—Pulse 100, softer; bowels regular; arm and elbow-joint much the same as yesterday. To go on with the remedy.

Fifth day.—Pulse 88, soft; the pain and swelling of the elbow have decreased rapidly since the last visit, and are now very slight; bowels still regular.

Eighth day.—No pain anywhere, but still some stiffness of the right elbow-joint, which prevents the complete extension of the arm. Bowels regular.

Twelfth day.—Dismissed, free from all complaint, and having the full use of his right arm, the elbow-joint of which has returned to its natural size. This patient required one dose of the compound rhubarb powder during the treatment.

Case III.—James S——, aged 20, a baker, much exposed to heat and cold in working at night; never before had a similar attack; has suffered for ten days pain and swelling of both shoulder-joints. Admitted into Naaman's ward on Sept. 10th, 1851, having great pain and swelling of both shoulders, but especially the left, so that he is very reluctant to attempt to raise the left arm. Pulse 108, hard; skin hot; tongue white and dry; much febrile excitement; bowels confined. Ordered six ounces of lemon-juice three times a day; fifteen grains of compound extract of colocynth to be taken at night.

First day after admission.—Bowels well relieved; pulse 82; less pain; to continue the lemon-juice.

Fifth day.—Pulse 80; pain and swelling much abated; to go on.

Eighth day.—No pain or swelling; is able to raise both hands above his head, and so far as regards the rheumatism, is well. Patient continues in the hospital in consequence of a swelling in the groin, which was opened two days after his admission, and is going on favourably. He required purgative medicine every alternate day whilst taking the lemon-juice.

Case IV.—John H——, aged twenty-two, single, a policeman, formerly a farm-labourer, with florid complexion, light hair, and strumous appearance; habits temperate. The patient was attacked with acute rheumatism three years ago; but has since had good health, until within the last fortnight, when he began to suffer from his present illness. The inner sides of each foot and ankle were affected, and then the elbows and hands became swollen, red, and painful, in which state he was admitted into Naaman's ward, No. 8, on July 9th, 1851.

There was then a murmur heard on the heart's systole; pulse 104, hard, strong, and incompressible; tongue coated and white, countenance congested; appetite gone; bowels costive; urine acid, specific gravity 1010, about one quart passed daily; perspiration profuse; pulse 92. Ordered six ounces of lemon-juice, three times a day.

First day after admission.—The pain and swelling of the elbows and hands decreasing; ordered to continue the remedy.

Fifth day.—The lemon-juice seemed to cause a little griping pain in the bowels, which were moved twice a day whilst its use was continued. The pulse 80; small, weak, and compressible. There remained not a vestige of pain nor swelling anywhere.

Tenth day.—Pulse still 80; no *bruit* heard now with the systole of the heart. The patient has been quite free from all complaint for several days. The pulse retains the same character as on the fifth day. Discharged cured.

Case V.—William B——, aged thirty-five, a married man, employed as a waiter, a native of Dorsetshire, who has lived in London for sixteen years; temperate, intelligent, with clear complexion, dark hair, and having excellent ordinary health. Five years ago, in the winter, he was laid up seven weeks with rheumatic fever. This had been his only illness since childhood. Admitted into Job ward, No. 18, on July 9th, 1851. The knees, ankles, and the small joints of the left hand swollen, somewhat red, heated, and very painful on the slightest motion; chest healthy; heart normal; appetite good; skin hot and dry; urine loaded with lithates, scanty, about a pint daily; bowels regular. Ordered four ounces of lemon-juice three times a day.

First day after admission.—To go on; the juice produced no uneasiness of the bowels or diarrhœa.

Eighth day.—The pain and swelling having almost entirely ceased, the patient was ordered sesquicarbonate of ammonia, five grains; decoction of cinchona, one ounce, three times a day; that he might recruit his strength.

Ninth day.—Much better; the joints are entirely free, except the knuckle of the middle finger of the left hand, which is swollen; and this the patient attributes to pressure of the walking-stick which he uses to assist him in moving about the ward. His knees fail him from mere weakness; but they are free from pain. He is considered nearly well of the rheumatism, but remains under treatment on account of inflammation of the right eye.

Case VI.—Thomas M——, aged twenty-six, single; a cooper, living in Commercial-road, Old Peckham; admitted into Naaman's ward, No. 14, August 20th, 1851. Aspect phlegmatic; has been a hard drinker, but has otherwise lived well. His general health has always been good. Had syphilis about six years ago, but it was not followed by any secondary symptoms. About five months ago, the patient was attacked with loss of power in the motor nerves of the right arm,—probably rheumatism,—for which he was admitted into St. Thomas's Hospital, and discharged cured three months afterwards.

About six weeks ago, he was attacked with general rheumatism, and from that time has been unable to work. He has not, however, been under medical care, and has not taken any medicine.

Present symptoms.—Complains of severe pain in the back of the neck and between the scapulæ, which is constant, but increased on the slightest movement. Pulse 99; tongue slightly furred; skin hot and dry; bowels regular; has slight pain in the head; sleeps well, and is not disturbed by the pain during the night. *Treatment.*—Six ounces of lemon-juice three times a day.

First day after admission.—The pain is much relieved. Pulse 76; skin moist. To go on.

The fifth day.—The pain is much less severe, and is intermittent. Pulse 72, soft; skin moist and cool; bowels regular. To go on.

Eighth day.—Complains of want of sleep; the pain is now only felt on walking, or moving the affected parts. Ten grains of Dover's powder every night, and repeat the lemon-juice three times a day.

Eleventh day.—Complains of pain in the head, probably caused by the opiate at night, which, as he now sleeps well, he is directed to omit, still continuing the lemon-juice.

Fourteenth day.—Patient can now move his head in any direction without the least pain. The uneasiness between the scapulæ, however, still continues when he moves his arm or walks. Ordered to omit the lemon-juice, to be dry-rubbed between the shoulders on alternate days, and to take a draught of magnesia, with wine of *colchicum in mint-water*, three times a day.

Sixteenth day.—Is now free from pain; and having remained so until the nineteenth day, he was discharged cured.

Case VII.—Samuel T——, aged sixty-six, married, living at 9, Pollen-street, Maddox-street, one of the pew-openers of St. George's, Hanover-square, a stout man, with fair, florid complexion and grey hair. Has been the subject of gout for twelve years, within which period he has had very severe attacks, at intervals of about two years and a half, which have stiffened his joints so that he can hardly hobble along.

On Tuesday, 15th July, 1851, having had for three days well-marked gout, with much pain in his left hand, he was recommended to try lemon-juice, and on the evening of that day he took two ounces. On the second day he took six doses, each of two ounces, at intervals of about three hours.

On the third day he took five doses.

On the fourth day four doses.

On the fifth day four doses.

On Sunday, the sixth day, in consequence of having to attend his duties in the church, he omitted the lemon-juice altogether.

On Monday, the seventh day, he took a wineglassful three times in the course of the day.

On the eight day, two wineglassfuls.

On the ninth day he took none.

On the tenth day he took one wineglassful, and then left the remedy off altogether.

His account of the effect is as follows:—

On the fifth day he began to get ease. On the following day his duties fatigued him much, but he had no pain. On the seventh day he felt a kind of shaking or throbbing in the hand, which lasted for several hours; the hand then gradually became pliable, having been stiff before, and from that time he has felt quite well, the few doses which the patient took the eighth and on the tenth day being merely taken by way of precaution for fear of a relapse.

His limbs, which had been long stiffened with gout, the last attack of which confined him to his bed for many weeks, have become more and more pliant, and he states that he has not felt so well for the last twelve years, having a good appetite, sleeping well, and being able to move about without pain, and with tolerable freedom. He says that the lemon-juice did not open his bowels, but that when he took twelve ounces in the day he had some griping pains. The patient has had no attack of gout up to the present time, Nov. 1st.

Case VIII.—Ellen ——, aged twenty, a florid young woman: whilst in service she met with an accident from the fall of a box, which struck her side. Two or three weeks afterwards she began to expectorate blood, and this continued for two months. She was then admitted into Lydia ward, on the 18th of June, 1851, and continued under treatment until Sept. 9th—viz., about three months.....During this period she continued to cough up blood almost without intermission, to the extent of from a quarter to half a pint daily. The catamenia, though regular, were scanty, and her complaint was regarded as dependent on imperfect menstruation.

Various remedies, as well astringents and sedatives as saline aperients, were tried, but with partial success; and although the repeated application of leeches to the side was more efficacious, still the bleeding recurred until the 18th of August, when she began to take three-ounce doses of lemon-juice, three times a day. From that time to the present (Oct. 14) she has had no return of the complaint. With this patient, as with all who have followed this treatment, the lemon-juice had the effect of lowering the power and frequency of the pulse.

Dr. Babington stated, in some remarks he made on the subject, that from the above cases, and many others not recorded here, he had come to the same conclusion as the original discoverer, Dr. Rees, namely, that *in acute rheumatism*, or as it is otherwise called, rheumatic fever, whatever be its severity, there is no remedy with which he was acquainted, possessing anything approaching the curative power of lemon-juice. Dr. Babington had never seen nor even read of such speedy cures by any means whatever, as were effected in the six cases which have just been narrated. Of these cases, it will be observed, that the first got well in three days,

the second in eight days, the third in eight days, the fourth in five days, the fifth in nine days, and the sixth in sixteen days. The seventh case, though not of the same complaint, is equally remarkable. Inveterate gout occurring in a man, sixty-six years of age, was materially improved, and a renewed attack was subdued by a few days' employment of this perfectly safe remedy; and in the eighth case an obstinate hemorrhage from the lungs was arrested and permanently cured by the same means.

Dr. Babington further remarked, that since Dr. Rees first recommended lemon-juice, it has been employed by many medical men, and, as he understood, with variable success. He was inclined to attribute occasional failures, of which he had himself met with several, to the fact, that according to his experience it is, as above stated, in acute rheumatism alone that the remedy proves so certain; whereas it has been administered in all kinds of cases.

Dr. Babington did not mean to affirm that it is not also very frequently useful in chronic rheumatism, and in gout, but only that its effects are far less obvious, and far less uniform, in these diseases. Failure, or at least a want of striking success, may also be ascribed to the circumstances that the doses administered have hitherto been too small.

It was only within the last four months, that he had ventured to prescribe more than from one to two ounces for a dose; and Dr. Babington believes that scarcely any one but himself has hitherto exceeded the latter quantity. He claimed no merit for having adopted a bolder practice, for the change resulted from the following circumstances.

A gentleman, who had consulted Dr. Babington several months before, called about severe rheumatic pains which he was experiencing. He was recommended to take lemon-juice. From some cause or other, however, the patient did not take it; and after an interval of several months, being again visited by his rheumatic pains, he applied to a chemist for relief. This person told him he ought to try lemon-juice, and it then flashed across his mind that this was the remedy which Dr. Babington had long before spoken of. The gentleman told the chemist he would follow his advice; but "what is the dose?" said he. The chemist replied, "*half a pint* (!) three times a day," and added that this was the quantity which he himself had taken for a similar attack, and with the greatest success. The patient, a gentleman of a resolute spirit, unflinchingly took the quantity prescribed, which, however, he found it somewhat difficult to swallow, and, according to his own account, in three days he was well.

Dr. Babington asked him what effect it had upon his bowels, and learned that the patient had been astonished to find that it did not disagree with him in any way, although his bowels were remarkably tender and easily put out of order. It being thus clearly proved that even eight or ten ounces of lemon-juice taken at a dose were not productive of any ill effects, Dr. Babington from that time felt warranted in increasing the quantity; and the remarkable effects he had witnessed, of which the cases above cited are but a few samples, were the fortunate results.

From the depression of the circulation caused by lemon-juice taken in large doses, Dr. Babington considers it highly probable that it will be found a valuable remedy in inflammatory diseases in general. He intends to take an early opportunity of trying it in pneumonia or pleurisy, as he thinks that it will act as beneficially in those diseases as antimonials, and much in the same way, with the exception of the nauseating effects—namely, by lowering the circulation and promoting perspiration.—*Lancet, Nov.* 8, 1851, *p.* 431.

7.—ON THE PHYSIOLOGICAL EFFECTS OF THE COLCHICUM AUTUMNALE.

By Dr J. M. Maclagan, Edinburgh.

One of the most remarkable effects of colchicum was discovered by Chelius of Heidelberg. He found that the uric acid contained in the urine of those taking *colchicum was* nearly doubled in the space of twelve days. In one case the urine

before taking colchicum contained 0·069 per mille of uric acid; four days after commencing to take the colchicum the proportion was 0·076; on the eighth day, it was 0·091; and on the twelfth day, it was 0·102. Chelius obtained the same results in other instances.

Dr. Christison examined the urine of a patient taking colchicum, and he found that in two days the quantity of urea was nearly doubled. In the urine before taking colchicum there was no deposit of *lithate of ammonia*. Its density 1020. It contained about forty-seven parts of solid matters in the thousand, and of this quantity twenty parts were urea. The specimens of urine passed on the first and second days after commencing to take colchicum were exactly alike. They were very turbid, and their turbidity disappeared with a gentle heat; the deposit was evidently *lithate of ammonia*. The density of the first was 1033·5, and that of the second was 1034, which are both very unusually high for urine not *diabetic*. As they were obviously identical in their nature, Dr. Christison only analysed the second. It contained seventy-nine parts of solid matters in a thousand; and of this quantity thirty-five were urea. Dr. Christison suspected that the quantity of urea was even greater, for not having added an excess of *nitric acid*, some of the *nitrate of urea* might have remained in solution.

Through the kindness of Dr. Hallidày Douglas, I had an opportunity of examining the effect of colchicum on the urine of a sailor, who was a patient in the Royal Infirmary. He was under treatment for secondary syphilis, but was otherwise healthy. I was permitted to give him a few doses of colchicum, in order that I might ascertain the physiological action of that agent on the kidneys, but before doing so I examined his urine. The density was 1025. It contained no deposit, nor was it affected by *heat* or *nitric acid*. It contained:—

Total solids,		27·500
Water,	972·500	
Urea,	12·360	
Uric Acid,	0·281	
Inorganic salts,	7·436	
Organic matter,	7·423	
Total	1000·000	

Here it will be perceived that both the urea and uric acid were slightly deficient, if we compare it with the standard of healthy urine, as given by Becquerel. Density 1018·9. Contains:—

Total solids,		31·185
Water,	968·815	
Urea,	13·838	
Uric Acid,	0·391	
Inorganic salts,	7·695	
Organic matter,	9·261	
Total	1000·000	

On the third day, after commencing to take colchicum, the urine was examined. It possessed a slight turbidity, which, however, was dissipated by heat. Density 1030. It contained:—

Total solids,		29·650
Water,	970·350	
Urea,	15·500	
Uric acid,	0·491	
Inorganic salts,	6.350	
Organic matter,	7·209	
Total	1000·000	

Here, it will be observed, the urea was increased by *one-fourth*, the uric acid nearly *doubled*, and the inorganic salts and inseparable organic matters were considerably decreased.

The urine was *again examined* on the sixth day after commencing to take the

colchicum, with the following results. Turbidity rather increased. Density 1034. It contained:—

Total solids,		33·460
Water,	966·540	
Urea,	18·341	
Uric acid,	0·750	
Inorganic salts,	7·436	
Organic matter,	6·933	
Total	1000·000	

Here, then, the physiological action of colchicum in increasing the urea and uric acid was well marked.

Having obtained these results from this case (which are only corroborations of many others), it was not considered justifiable to proceed further with the administration of colchicum with this patient.

It has been supposed that under the use of colchicum a remarkable change takes place in the system,—namely, that the uric acid becomes converted into urea; but this has not at all been substantiated, and from the above cases of Chelius, and the analyses which I have just noticed, we must be led to suppose that no such change occurs, but that an increase in both those principles is the result.

Dr. Graves states, that the beneficial action of colchicum is not owing to its producing a more rapid excretion of *lithates* through the kidneys, but to the remarkable property the plant possesses of altogether putting a stop to the morbid formation of *lithates*.

Dr. Gairdner says, that he has always found that the increase of urea was accompanied by a corresponding diminution of the *urates* in the urine. But, from the above experiments I am inclined to believe that both of these suppositions are erroneous.—*Monthly Journal of Med. Science, Dec.* 1851, *p.* 513.

8.—ON THE EFFECTS OF COLCHICUM IN ACUTE RHEUMATISM.

By Dr. J. M. Maclagan, Edinburgh.

[Dr. Garrod states that in acute rheumatism no more urea and uric acid are to be found in the blood than in health; this being a very minute quantity. Dr. Maclagan differs from this opinion, believing both to exist in an increased quantity. He says:—]

In the cases which I am about to relate, and which fell under my own observation, I shall endeavour to prove that such is the case, and to show, from the analysis of blood and urine, which I made both before and after the exhibition of colchicum, that the remedial agency of this medicine is due, partly at least, to its power of eliminating urea and uric acid from the blood, and increasing their quantity in the urine.

The first case was that of a girl, under Dr. Wright's care, in the Royal Infirmary. On the 13th of October a small quantity of blood was subtracted, analysed, and found to contain:—

In 1000 parts of blood	0·507 Urea.
" "	·864 Uric Acid.

The urine was examined at the same time. It contained:—

Total solids,		28·568
Water,	971·432	
Urea,	10·496	
Uric acid,	·257	
Inorganic salts,	7·461	
Organic matter,	10·354	
Total,	1000·000	

Colchicum, in combination with muriate of morphia, was then administered.

The urine was again examined on the 18th October, being the fifth day. It was found to contain:—

Total solids,	31·459

Water,	968·541
Urea,	12·312
Uric acid,	·421
Inorganic salts,	8·231
Organic matter,	10·495
Total,	1000·000

The urine was again examined on the 22nd October, or ninth day. It contained:—

Total solids,	35·613
Water,	964·387
Urea,	13·984
Uric acid,	·598
Inorganic salts,	9·401
Organic matter,	11·630
Total,	1000·000

After twelve days' constant use of the colchicum, a small quantity of blood was procured for examination. Now, however, not the slightest trace either of urea or uric acid could be detected in so large a quantity as 3500 grains.

The urine was examined at the same time, and was found to contain:—

Total solids,	34·554
Water,	965·446
Urea,	14·561
Uric acid,	·737
Inorganic salts,	9·649
Organic matter,	9·607
Total,	1000·000

The colchicum being still continued, the urine was again examined on the eighteenth day, and found to contain:—

Total solids,	38·128
Water,	961·872
Urea,	17·635
Uric acid,	1·034
Inorganic salts,	9·999
Organic matter,	9·460
Total,	1000·000

The next case to which I shall refer was treated by myself in the New Town Dispensary. I had only an opportunity of examining the blood in this case once, namely, before colchicum was taken. It then contained:—

In 1000 parts,	1·416 Urea,
"	·691 Uric acid.

Before taking colchicum, the urine contained:—

Total solids,	23·479
Water,	976·521
Urea,	6·358
Uric acid,	·097
Inorganic salts,	7·333
Organic matter,	9·691
Total,	1000·000

The urine was again examined on the fourth, ninth, and thirteenth days respectively, and contained:—

On 4th Day.

Total solids,	24·538

Water,	975·462
Urea,	9·103
Uric acid,	·231
Inorganic salts,	8·693
Organic matter,	6·511
Total,	1000·000

On 9th Day.

Total solids,	26·322
Water,	973·678
Urea,	12·981
Uric acid,	·497
Inorganic salts,	9·400
Organic matter,	3·444
Total,	1000·000

On 13th Day.

Total solids,	27·466
Water,	972·534
Urea,	16·824
Uric acid,	·936
Inorganic salts,	7·203
Organic matter,	2·503
Total,	1000·000

These are the analyses of two very favourable cases, in which it will be seen, that the urea and uric acid are increased in proportion to the time that the medicine is continued. I have selected these two cases from many others, on account of their showing the increase more gradually. In all the cases, however, in which I have analysed the urine, the great increase was distinctly marked, although perhaps not in so regular proportion.

One other case I would wish to mention, in which the *urea* and *uric acid*, although they increased after a few days' use of the medicine, did not continue to do so subsequently in the same proportion.

The analyses were made before taking colchicum, and on the third, sixth, and tenth days after its exhibition.

Before taking Colchicum.

Total solids,	25·636
Water,	974·364
Urea,	7.684
Uric acid,	·129
Inorganic salts,	8·421
Organic matter,	9·402
Total,	1000·000

On 3d day.

Total solids,	27·479
Water,	972·521
Urea,	11·158
Uric acid,	·300
Inorganic salts,	8·303
Organic matter,	7·718
Total,	1000·000

On 6th day.

Total solids,	27·907

Water,	972·093
Urea,	15·660
Uric acid,	·570
Inorganic salts,	6·500
Organic matter,	5·177
Total,	1000·000

On 10th day.

Total solids,	28·426
Water,	971·574
Urea,	15.730
Uric acid,	·582
Inorganic salts,	7·351
Organic matter,	4·763
Total,	1000·000

These analyses will show to what extent colchicum will alter the renal secretion, by supplying it, when deficient of its normal constituents—*urea* and *uric acid*—from the blood.

I am inclined from all this to believe, that to this property of colchicum its remedial action is in a great measure to be referred. Further analyses of the blood may, however, be thought necessary, but neither time nor opportunity have afforded me means for this purpose. The thirteen analyses of the urine recorded above are only a few selected from many others made by me, and were effected according to Becquerel's method.

Colchicum has been employed in all forms of the disease, but it appears to me more particularly useful in *articular rheumatism*. In Dr. Watson's words, "our wishes and our expectations from colchicum are often doomed to be defeated. I believe that in proportion as the synovial symptoms predominate, or mix themselves distinctly with the fibrous—in proportion as the disease approaches in its characters to gout—you may expect to be successful with colchicum. Large doses are not requisite; twenty minims of the *tincture* or the *wine* may be given every six hours, until some relief is obtained; or a grain of the *inspissated juice*, or of the *acetic* extract of colchicum, every four hours. Under this treatment the disease sometimes vanishes within three or four days; the medicine producing sickness and purging, and the rheumatism or the rheumatic gout rapidly declining. Occasionally the same favourable event takes place, although there has been no disturbance of the stomach and bowels."

The cases in which colchicum appears to me to be more pre-eminently useful are those in which the disease attacks the joints, and is of an erratic character—*e. g.*, suddenly disappearing from one joint, and as suddenly appearing in another; these, I believe, are the cases in which the heart and other internal organs are chiefly affected, and probably if the active operation of colchicum is secured early, these formidable secondary diseases are less likely to occur.—*Monthly Journal of Med. Science, Jan.*, 1852, *p.* 21.

9.—ON THE EFFECT OF COLCHICUM IN DROPSY.

By Dr. J. M. Maclagan, Edinburgh.

In *dropsy* succeeding to *scarlatina* I have frequently found colchicum of much service, especially in cases where the urine is much suppressed, and where comatose symptoms are present. The accession of *coma* may easily be ascribed to the accumulation of *urea* in the blood; and the power which it has been shown that colchicum possesses of replacing the *urea* in natural, and often superabundant, amount in the urine, seems to point it out as a useful remedy in this and other diseases in which *suppression of urine* and *coma* co-exist. In a case of scarlatina which I attended along with my friend Dr. A. Christison, now of the H. E. I. C. Service, and where the urine was totally suppressed, and the symptoms of coma were present, the *acetic extract of colchicum* was used with complete success. Diluents and ordinary diuretics were freely employed when the case was first seen,

with the effect of causing a slight secretion of urine of low specific gravity. Having suggested that colchicum might be found of some service, and being anxious to observe its effects, both as a diuretic and as an eliminator of urea, I examined the urine before its exhibition, The results were as follows:—

Urine examined on 10th April.

Total solids,	35·795
Urea,	2·427
Uric acid,	a trace
Inorganic salts,	13·510
Organic matter and water,	969·573
Albumen,	14·490
Total,	1000·000

The *acetic extract of colchicum* was ordered on the 11th April, and the other medicines discontinued. On the 12th, the comatose symptoms were considerably abated; urine, of a normal density, was passed in tolerable quantity, and was examined again on the 13th, two days after the exhibition of colchicum. It contained:—

Total solids,	30·659
Urea,	7·500
Uric acid,	0·480
Inorganic salts,	8·718
Organic matter and water,	975·359
Albumen,	7·943
Total,	1000·000

On the evening of the 14th, the comatose symptoms disappeared; urine, of normal density, was passed in proper quantity; the dropsical effusion and anasarca completely gone. On the 15th, considerable diarrhœa had set in; the colchicum was stopped, and the urine again examined. It contained:—

Total solids,	27·972
Urea,	13·573
Uric acid,	0·814
Inorganic salts,	7·431
Organic matter and water,	978·182
Total,	1000·000

The analysis of the urine in this case will show the powerful influence which colchicum possesses in altering the renal secretion, and of how much service, as a remedy, it may be in cases of threatened poisoning by *urea* in the blood. I believe, in all cases where *albumen* and *urea* appear to be vicarious, and where *coma* supervenes, evidently from the accumulation of the latter principle in the blood, that colchicum will prove to others of as great service as it has already done in the small experience I have had of it.

Two other cases of a similar nature, in which the urine was examined at regular intervals, were treated in the same manner as that which I have related, with precisely the same result.—*Monthly Journal of Med. Science, Jan.*, 1852, *p.* 9.

10.—ON FATTY DEGENERATION.

[Mr. BARLOW read an interesting paper on this subject before the Medical Society of London:—]

He began by insisting on the great importance and extent of the whole question of fatty degeneration, which had naturally interested so many observers, from the vast number of pathological consequences which it, and it only, could explain. The subject was to be viewed comprehensively, and as a whole, and not as illustrated by any particular organ or part. It could not be separated, unless by the imagination, from the yet larger question of atrophy, of which indeed, as Mr. Paget had said, it was a manifestation. Fatty degeneration was the consummation and result of atrophy—a word which should be used as denominating the slightest impair*ment of nutrition*, as well as that obvious wasting of parts which paralysis, phthi-

sis, and many chronic maladies presented, in forms more or less striking. Seeming hypertrophy and real atrophy co-existed: muscular fibres might seem enlarged to the eye; but, viewed with the microscope, might be found degenerated into fat, as had been long shown in reference to the hypertrophied heart, and lately by Mr. Hancock in relation to the (so called) hypertrophied bladder. Bulk was in these cases no measure of power, but just the contrary. The author thought that the general subject of fatty degeneration might be best pursued by looking at the conditions known to be essential to perfect nourishment, and then fairly tracing the results of their failure. The degeneration might be produced—first, by a wrong or defective state and composition of the blood; second, by an insufficient supply of blood; third, by a deranged or obstructed influence of the nervous system; fourth, by an imperfect, unhealthy, or declining state of the part to be nourished. These conditions were, in every respect, the reverse of those which Mr. Paget had specified as most important to healthy nutrition. The author treated these points in detail, and the following is a mere outline of his remarks:—

I. The state of the blood in reference to nutrition had been noticed by Harvey in an emphatic manner. It was modified by age and varied by disease to a remarkable extent. It had been found deficient in corpuscles, in anæmia (or spanæmia, as Simon termed it), hemorrhage, and some cases of granular degeneration of the kidney; but much on this subject was to be inferred from reasoning on certain obvious conditions of the system; nor could it be expected that analysis, however ingenious and subtle, could be expected to cope with half the difficulties of the question. The blood might be said to grow old; he believed the climacteric disease, as Sir Henry Halford had denominated it, to be a mere sign of degeneration—oftentimes of fatty degeneration, or that chiefly. This kind of decay, happening late in life, and gradually loosening its tenure, could hardly be called morbid or abnormal, being altogether as much in the order of nature as the fading of a leaf at the autumnal season. But decay was impatient, and anticipated time: premature degeneration would occur from various general and local pathological states, in many whereof the blood was to a certainty most seriously involved. The observations of Dr. Omerod and Dr. Quain upon the general relation of wasting maladies to this destructive process, as well as those made by Mr. Gulliver, on its occurrence in various parts and states, were referred to at length. The author knew, from his own observations, the vast practical bearings and high interest of this part of the subject. Fatty degeneration was very frequently the most perilous complication, and it was necessary to watch for, and if possible to detect it. Persons sometimes during chronic maladies would turn pale, and expire in a moment: the heart was at fault; degenerated to the utmost, it could act no more. It was to be suspected that the hemorrhage of disease, as well as artificial losses of blood, had very often assisted in inducing, and quite as often greatly aggravated, this downward change. Cases of fatty degeneration had occurred at six and eight years of age, and Dr. Snow Beck had observed an instance of degeneration of the small cerebral blood-vessels in a child aged one year and seven months; he had detected minute granules in their coats, but did not consider it certain that they were oil-globules, as they had not been tested by dilute hydrochloric acid and ether.

II. The question of inadequate supply of blood was, for the most part, inseparable from that already treated of. Pure anæmia could never last long; abstraction of blood even, if too frequent, led to the spoiling of what remained; yet some practitioners had bled the palest and most bloodless, on the bare imagination of inflammatory action, but not thus could be remedied confirmed anæmia, or atrophy repaired, or degeneration arrested. The changes throughout the arteries and their branches which led to local obstructions of the circulation, needed fuller inquiry; those obstructions of the heart, as had been well shown by Dr. Quain, acted in the manner of partial ligatures, and led to local anæmia and decay; or they might proceed the length of completely cutting off the supplies of a part. Whether narrowing of an artery would cause local atrophy or death, depended on the condition of the neighbouring vessels, which might be too degenerated themselves to enlarge and compensate for the withdrawn supply. The same remark applied to ligatured arteries in cases of aneurism. Arterial degeneration might be so extensive, and especially if the heart were fatty and feeble, as to prevent the circulation being carried *on*. *The peculiarity of the cardiac circulation, noticed by Mr. Swan,*

and remarked on by Dr. Quain, as favouring fatty degeneration of the heart, was observed upon. There was no free communication between the branches of the coronary arteries, and the conservative influence of anastomosis was absent. Harvey had dwelt most fully on local modifications of the general circulation. The author thought that some local deaths which had been referred to obstructions of an artery or large branch of it, had really depended on the arrestation of the streamlets in its smaller ramifications. All general and local defects of circulation were to be considered perilous in proportion as the general nutrition might be impaired.

III. The question of nervous influence, in relation to the failure of nutrition and fatty degeneration, was extremely complex, but there could be no doubt whatever that its derangement or withdrawal had a large share in the production of atrophy and its results. Roston, Lallemand, and others, had referred to cases of softening of the brain, which they partly attributed to grief and anxiety. But though nothing was more certain than that the mind so deranged the vital function as to waste the body and "fret it to decay," there was nothing harder than to measure its influence,—to separate it from other simultaneously acting causes of destruction, or to define the exact part which it played amongst them. The withdrawal of nervous influence by mechanical means, such as those seen in the paraplegia of disease or fracture of the spine, was to be considered by itself; the mind could not reach the affected parts; volition and emotion were withdrawn; and the atrophy which followed was to be explained more readily than that which happened in parts yet open to mental impressions. The state of the mind often injured nutrition, in an indirect manner, by withdrawing the body from those exercises and diversions which constituted main sources of its vigour; by depressing and interfering with the heart's action, so that the blood was circulated with insufficient frequency and force; by modifying the respiration so extremely that the oxygenation of the blood was less perfect than it should be; by seriously interfering with the digestive function, the disorders of which spread through the body; and, oftentimes, by banishing or breaking the sleep, which had been called so finely, "chief nourisher in life's feast:" but he doubted not that, in many cases, from what might be observed of the effect of the emotions upon different secretions in time of health even, that there was frequently, besides all this, an indirectly depressing influence exercised by the mind, both locally and generally, on the function of nutrition; and he thought it probable that it might at times aid greatly in leading to degeneration, and in aggravating it where it already existed. But he would refer to some observation by Mr. Paget, in reference to matters more or less touching this subject; and also to Dr. Holland's chapter, contained in his 'Notes and Reflections,' on the bodily organs.

IV. The fatty degeneration of parts, as affected by general and local defects of assimilation, was one of the most difficult yet important divisions of the subject. Each tissue might be termed a centre of appropriate and discriminative nutrition. It had a special life of its own, though that were in the circle of, and dependent on, the general vitality. It had, doubtless, hereditary strength or weakness, such as affected the entire organism. It had its own circulation, and its own nervous influence, liable to especial impairments; it might be subject to premature death, or be found, when the body as a whole had died, with a structure perfect to the minutest line. The healthier a part was, the likelier was it so to remain; and the converse was certain. The least atrophy was an approach of death, no matter whether it concerned a fibre, or the complex anatomy of all the frame. But mere atrophy was recoverable; and though parts of an organ had degenerated irrecoverably, the surrounding structure might be restored to vigour, and compensate effectually for the loss sustained. The only part in a degenerated condition which could be watched by the eye, was the cornea; and a case of great interest had been communicated to the author by Mr. Charles Simpson, in which the arcus senilis, which had been both well and frequently observed, had disappeared. He agreed with what Dr. Quain had written with respect to the treatment of the degenerated heart, and the hope, in some cases, of arresting the changes in it. Reparation there could not of course be seen, but the undestroyed fibres might enlarge and strengthen, and its beats become firmer, and its sounds more audible. In the *young, the middle-aged,—nay,* in some quite old—in all, indeed, who had much

sound structure left,—fatty degeneration might probably be counteracted by a new vigour infused into the body, and by certain changes in the atrophied tissues which adjoined or bordered on those which had perished. The author adopted Dr. Quain's theory of the process whereby fatty degeneration was effected, and referred to a suggestion made by Dr. Williams respecting its nature. Dr. Quain had found, by accident, changes produced in the hearts of the dead similar to those which often happened in the living under circumstances of low vitality, of a feeble or deficient circulation, of impaired nervous influence, of structure long damaged by insidious atrophy. It was the power, or rather the combined powers, of life, however vague we might term the expression, which preserved us in integrity, and hindered the changes which implied death. An extract was read from Dr. Quain's memoir touching this point. How long the tissues were preserved healthy in some cases, was not less remarkable than their premature decay in others. Mr. Canton had given him a minute description of the state of the heart-fibres of a man aged 103, which was drawn by Dr. Quain after most careful examination; and it was only in a spot or two, and to an inconsiderable extent, that any of the fibres were found degenerate; but what was most interesting, viewed in connexion with this state of the heart, was, that the cornea was free from the arcus senilis, and microscopically examined, only presented a few fat-granules scattered here and there. The specimen was, through Mr. Canton's kindness, placed before the Fellows, and they might compare it with another, in which the arc had been obvious, showing the true structure of the cornea thickly crowded with countless fat-granules. The author referred to some recent observations made upon the subject of fatty degeneration, but especially to those which had been made respecting the fibres of the uterus, and the vessels of the placenta. He also directed attention to a most interesting contribution of Dr. Dittrich, who had found a fatty degeneration of the vessels of the lungs in some cases of pulmonary apoplexy, similar to that already discovered by Mr. Paget (and to which the author had, on a previous occasion, specially called the attention of the Society) in those of the brain. Dr. Kirkes had kindly given him an abstract of the principal conclusions. The most important was that there was "a fatty metamorphosis of the walls both of the larger and especially the smaller and smallest vessels." This occurred in the "large majority" of cases of pulmonary apoplexy. Dr. Charles Shearman had found most distinct groups of fat-granules lying in the substance of the coats of a small vessel of a degenerated heart. The "diameter of the vessel was $\frac{1}{1\,7\,1\,5}$ inch." The author had the opportunity of observing it. Marked degeneration of the cerebral blood-vessels, great and small, was found also. The author, after taking a general view of the important researches of various inquirers, asked the Society whether a grand advancement had not been made in a subject which bore largely and intimately, not only on the science but the practice of medicine.

[In the discussion which followed the reading of this paper, Dr. QUAIN remarked that]

The union of oil with albumen was a primary step in the process of nutrition, and the presence of oil with albumen, constituting the fatty granules which appear in fatty degeneration, was, so to speak, the evidence of final decay. These combined elements originally enter the blood from the chyle vessels. In that fluid they soon disappear. They enter under the influence of what we call the organizing power into the composition of the tissues and textures. Here, under the continued influence of this power they undergo changes, they are removed, and a fresh supply afforded. But when this influence is weakened, this process of supply and removal is interfered with, the tissues decay and return to their primitive form.

[Mr. BARLOW also further said]

The fact that apoplexy was in many cases the effect of degeneration of the cerebral blood-vessels, and not the mere congestion and escape of blood from healthy channels, was ever to be borne in mind in the treatment of its prevention. Some cases of threatening organic apoplexy would still require blood-letting, no doubt; but in many cases it was to be resorted to with caution, and in some it would be found altogether unjustifiable. Again, it was one thing to avert an attack, and another, having averted it, to keep the system low and depressed for some long, unlimited period, and so impair everywhere the process of nutrition.

Congestion of the brain was dangerous, no doubt, where its vessels were atrophied; but in the endeavour to get rid of fulness or distension, one might be very apt to increase that fatty degeneration which particularly made such fulness and distension to be dreaded. In all that Dr. Quain had said in his memoir of the fatty heart as a most frequent occasion of sudden death by fatal syncope, he agreed most fully. It had been too common for practitioners to pronounce that persons had died of apoplexy simply because they knew not the reason of dissolution. The extreme suddenness of the death should, in many cases, have prevented their conclusion. Persons rarely turned pale and died momentarily from apoplexy, as they did not rarely from the fatty heart. Then cardiac had been considered as apoplectic coma. Most fatal mistake! To treat the former by blood-letting, adopting a routine and perilous practice, was most likely to annihilate at once. And always it must be considered that degeneration of the brain and heart not rarely co-existed; and where they did so, and the latter was extreme, there was always risk in bleeding to syncope.—*Lancet, March* 27, *p.* 308, *and April* 3, 1852, *p.* 334.

11.—ON LEUCOCYTHEMIA, OR WHITE CELL BLOOD.

By Professor BENNETT, Edinburgh.

[From the results of analysis, it would appear that the chemical constitution of the blood, in cases of leucocythemia, consists in an excess of the fibrin and diminution of the corpuscles, while the serous solids undergo little if any diminution. In seven out of nine analyses, the fibrin exceeded the normal amount. In other seven analyses, the fibrin varied from 3·2 to 7·08 parts in a thousand. In all the analyses the blood corpuscles were under the normal standard.]

Leucocythemia viewed in relation to Inflammation. The essential phenomenon of inflammation is exudation of the healthy liquor sanguinis, through the walls of the capillaries. The circumstances which lead to that exudation, though now tolerably well understood, have been differently explained by various writers. Thus the occasional and accidental accumulation of the colourless corpuscles within some of the smaller vessels was considered by Drs. Addison and J. C. B. Williams as an important, and even essential, part of the process. The latter author observes:—"It seems, then, to be well established that an essential part of inflammation is the production of numerous white globules in the inflamed vessels; and that the obstruction of these vessels is mainly due to the adhesive qualities of these globules." Shortly after these views were published, I made the following statement:—"Without denying the occasional accumulation of these lymph corpuscles in certain vessels, I must record my conviction, that inflammation, accompanied by complete obstruction, may be frequently occasioned, independent of any such phenomena." Numerous observations, again and again repeated, having fully convinced me that excess of colourless corpuscles had nothing to do with the stoppage of the blood in inflammation, I concluded an account of these with the following passage, in 1847:—"It may be concluded, then, that there is no increase in the white corpuscles in inflammation,—no crowding together of them, so as to produce obstruction of the vessel," &c. Notwithstanding these observations, however, Dr. Williams pertinaciously maintained this doctrine in the second edition of his work, published in 1848, observing, in allusion to the obstruction of vessels in inflammation:—"The chief cause of obstruction seems to be comprised in the two circumstances—the increased production of the white globules, and their remarkable disposition to adhere to the walls of the vessels and to one another."

This theory, which never reposed on accurate observation even in frogs, may be considered to have received its *coup-de-grace* by the discovery of leucocythemia in man. Here the colourless corpuscles *are* increased in number in the smallest vessels, and yet, instead of a universal inflammation, persons live in that condition for months and years, without any obstruction of the vessels whatever. Next to the discovery of what is new, the progress of science is most advanced by the expulsion of the erroneous observations and imperfect theories which encumber it.

Neither can the view of Mr. Wharton Jones, who considered inflammation to

depend primarily upon increased spissitude of the blood, and adhesion of the coloured corpuscles to one another and to the vascular walls, be considered tenable. The facts recorded in the first part of this memoir demonstrate that in one instance the fibrin was augmented to 7·08 parts in a thousand, in another it was increased to 6·0 in a thousand. On examining the blood immediately after its abstraction from the living body in several cases of leucocythemia, the coloured corpuscles were seen to aggregate themselves together in the manner so accurately described by Mr. Wharton Jones. The same facts were observable in Cases 9, 28, and in other instances; but in none of them were the smaller vessels and capillaries obstructed, or the phenomena peculiar to inflammation induced.

On the other hand, every known fact convinces me, and the progress of science only adds strength to my convictions, that we must ascribe the ultimate cause of inflammation to a derangement of those forces which regulate the nutritive powers of the economy, and that the only correct definition of inflammation itself is—an exudation of the normal liquor sanguinis. It is in vain that physiologists seek in the alterations of the vessels on the one hand, or morbid changes of the blood on the other, for the primary cause of this important condition. Facts prove that both are more or less affected, and also show that neither the one change nor the other, nor the two combined, constitute inflammation. The vital properties of the tissues (understanding by these the unknown conditions necessary for carrying on the nutritive processes) are in all such cases deranged, and such alteration is the cause of the changes which have been referred to, and not the effect.

Leucocythemia viewed in relation to Purulent Infection. That morbid condition, so much dreaded by surgeons and obstetricians, in which typhoid fever comes on after severe accidents or parturition, accompanied with purulent infiltration, or multiple abscesses, in one or more organs, has received different explanations. The various observations and experiments performed with a view of elucidating this subject in modern times have led to the four following theories:—

1. That this condition is owing to an admixture of the blood with pus (pyohemia of Piorry), and that the pus corpuscles being larger than the coloured ones of blood, are arrested in the minute capillaries, and give rise to secondary abscesses.

2. That it is owing to the presence of any irritating body, which cannot be eliminated from the economy, producing capillary phlebitis.

3. That it is dependent on a property possessed by pus of coagulating the blood.

4. That it is caused by the presence of a peculiar poison which contaminates the system.

All these views have been maintained with much ingenuity, and they are all supported by experimental and clinical researches. A knowledge of the circumstances previously detailed concerning leucocythemia will enable us to criticise these doctrines from a new point of view.

With regard to the first theory, it must, I think, be granted by all those who have examined the blood in leucocythemia, or will study the figures in the first part of this memoir, that no difference whatever can be detected between the colourless cells of the blood and those of pus. Their general appearance, size, structure, and behaviour, on the addition of re-agents, are identical,—indeed so much so, that in the first case I observed in 1845, I could not resist the conclusion that the blood was crowded with pus cells. It follows, that all explanations of purulent infection founded upon the mechanical impaction of these bodies in the minute capillaries must be erroneous. Some of these colourless corpuscles have been observed much larger than ordinary pus corpuscles. In one instance—a man still living—many of them were twice as large, and although this may in some measure be owing to endosmosis of serum, there can be little doubt that they must have exceeded the usual size of pus cells. In Case 2, also, it was observed that several of the colourless cells were larger than the average, and yet the circulation went on, and every drop of the patient's blood contained hundreds of these bodies. The first theory, then, is no longer tenable.

Neither does there seem to be anything peculiar in the substance of the good and the laudable pus, which necessarily leads it to poison the blood; for it is a matter of common observation, that large abscesses are absorbed and eliminated without occasioning so-called purulent infection. In all such cases, the pus corpus-

cles must, in the first instance, be disintegrated and reduced to a fluid condition; still the matter or substance of which they were composed passes into the blood. Hence, leucocythemia proves that corpuscles, identical in form, size, structure, and chemical composition with those of pus, may float in the blood and circulate innocuously; and the well-known fact of the absorption of abscesses demonstrates, that pus, when healthy, is not associated with any poisonous properties. If, then, the fever and other marked symptoms are owing to pus, it must be pus possessing properties wholly different from that which is generally called good or laudable.

The second explanation was advanced by Cruveilhier, who, on injecting mercury, ink, and other substances into the blood of a living animal, found that the multiple abscesses were formed wherever these accumulated. Hence impaction of some substances, and consequent local inflammations, *may* lead to abscesses; but that such is not the necessary result of admixture of pus with the blood, is proved not only by the previous observations, but by numerous experiments of Lebert and Sédillot, in which the animals recovered.

The third doctrine was advanced by Mr. Henry Lee, and resulted from observing that when pus was mingled with recently-drawn blood, it coagulated more rapidly and more firmly than under ordinary circumstances. This observation he connected with the well-known fact, that phlebitis was often associated with coagula causing obstruction of the veins. Now it is worthy of remark, that in decided cases of leucocythemia the blood is more highly coagulable when drawn from the arm, and after death it often presents firm coagula filling the vessels, as in Case 1. The same occurred in Case 2; and yet, during the life of the patient, the blood, loaded with the colourless corpuscles, rolled through the vessels without impediment or the formation of coagula. It does not follow, then, because dead pus is mingled with recently-drawn blood about to coagulate, that therefore it should induce coagulation of living blood in the vessels of an animal. Indeed, numerous experiments by Lebert and Sédillot show that such does not take place; for, although in some cases death followed, in others the animals lived, and the pus corpuscles were dissolved. Hence, although the fact to a certain extent must be admitted, that when pus is mingled with blood the coagulum formed is more firm, it by no means follows that it produces coagulation of *living blood*, and is the cause of phlebitis or purulent infection.

The fourth theory seems to have been maintained by A. Boyer and Bonnot, who believed good pus to be innocuous, and the bad effects occasionally produced to depend on its becoming putrid, or being otherwise altered. This view was also more or less supported by Darcet and Berrard, who, in order to explain the undoubted effects of putrid substances when injected into the veins, separated pyohemia from purulent infection. But as pus corpuscles do not alone cause the symptoms, it is certainly more probable that, in all cases, there must be a toxic effect associated with pus when it proves mortal. Dr. Millington has shown, in repeating Mr. Lee's experiments, that putrid fluids prevent coagulation of the blood, and that the coagulum caused by the addition of pus is more perfect the fresher the purulent matter is. This fact is opposed to the idea, that multiple abscesses are induced by the coagulation, but corresponds with what is observed after death in cases of purulent infection. When, therefore, we consider the typhoid nature of the symptoms so similar to that of certain animal poisons; the multiple abscesses so analogous to what occurs in glanders, plague, syphilis, variola, &c.; and the undoubted fact, that the blood may be loaded with corpuscles in every respect identical with pus cells, without causing these symptoms, the irresistible conclusion is, that these effects are not owing to pyohemia, but to an animal poison.

This view has been opposed on the ground that fresh pus, to all appearance healthy and without odor, has yet caused the death of animals. But what sensible property distinguishes the pus of the vaccine from the small-pox pustule, and either of these from healthy pus? And yet how different their effects when introduced into the blood! The subject of animal poisons is certainly obscure; but we advance our knowledge by attributing purulent infection to this cause, rather than in considering it to be the mere mixture of pus with the blood, or a so-called pyohemia.

Leucocythemia viewed in relation to Phlebitis. In none of the cases of leucocy*themia* could phlebitis, though carefully looked for, be anywhere discovered.

Although, in some instances, the clot was firmly coagulated, its colourless portion of a dull colour, very friable, and containing a multitude of corpuscles identical with those found in pus, nowhere was it adherent to the vessels. This was well observed in Case 1, in which the veins everywhere presented their normal transparency and thickness, notwithstanding the alteration in the blood. In phlebitis the effects are different. The vein is more or less thickened, the coagulum inside adherent, and obstruction of the calibre of the tube occasioned. From the numerous cases of phlebitis observed, especially when it originates in the uterine veins, the same general symptoms are produced as in the so-called cases of purulent infection. This indeed has been considered by many as the source of the pus corpuscles which mingle with the blood. But it is by no means shown, that, under such circumstances, the pus corpuscles actually circulate in the blood, much less that, if they did, the fatal result can be attributed to them. On the other hand, from the epidemic nature of the disease in puerperal women, and from its contagious character, a point which seems to be well established among practical obstetricians, it is more probable that here also a toxic effect is occasioned, which operates on the blood altogether independent of the pus corpuscles.

There can be no doubt that when, owing to phlebitis, a coagulum forms in the vessel, and obstruction of the blood occurs, that the clot softens, and is converted into pus. I have frequently seen such softened clots in veins, and on the internal surface of the cardiac cavities, to be composed of colourless cells, presenting all the characters of pus corpuscles, floating in a slightly molecular fluid. In most cases these corpuscles are prevented from entering the circulation, on account of firm fibrous coagula existing between the diffluent portion of the clot and the moving blood. But it is maintained, that occasionally the whole suppurates, and, on joining the circulation, causes the symptoms of purulent infection. If so, I argue that the effect must depend upon either the toxic power of such pus, or upon fragments of the coagulum being carried into the circulation, and acting mechanically, as the mercury did in the experiments of Cruveilhier. This point, however, in the history of phlebitis, requires further investigation, as well as the separation of such mechanical effects, should they occur, from the poisonous influence of altered or putrid pus.

From all that is known of the morbid anatomy of phlebitis, of the symptoms it occasions, and of the absence of these in cases of leucocythemia, it follows, not only that these symptoms are not occasioned by the circulation of colourless corpuscles in the blood, but that the conclusions formerly arrived at, as to the origin and physiological importance of these bodies, are correct.

On the relation between Morbid Conditions of the Lymphatic Glandular System and of the Blood. It is a matter of common observation, that the lymphatic glands and vessels swell in the neighbourhood of an irritating wound, and that the former are especially liable to become the seats of cancerous and tubercular matter, apparently the result of absorption from primary sores or lesions. The nature of this enlargement in lymphatic glands has not hitherto been very clearly understood. Generally speaking, it is attributed to secondary inflammation, which, if not subdued, advances towards suppuration, and in its turn becomes the source of similar lesions in the next series of lymphatic glands.

On examining glands which become enlarged from the result of irritation from a neighbouring ulcer, we find them to be soft, and to yield readily on section, a dirty turbid fluid. If we examine this fluid under a magnifying power of 250 diameters linear, we find it to be crowded with naked nuclei and the cell elements of the gland, some of which last are frequently enlarged, and contain a considerable number of nuclei. It would appear that, under these circumstances, the nuclear and cell elements not only increase in number, but that some of the latter assume a power of development which they never present in a state of health. For instance, instead of there being one nucleus, it multiplies fissiparously, so that there are two, four, or even a greater number.

This condition is remarkably well observed in the enlarged mesenteric glands which accompany typhoid ulcerations in the intestines. They are then greatly distended, varying in size from a hazel nut to that of a hen's egg. They are externally vascular, of a bright red or purple colour, are soft and pulpy to the feel,

and, on section, present a slightly granular surface, of greyish or fawn-yellow colour, and frequently exhibit commencing softening. They are friable, and yield a greyish or dirty purulent-looking fluid, on examining which with a magnifying power of 250 diameters, it will be found to contain numerous cells, generally spherical, varying in diameter from the 1–150th to the 1–35th of a millimetre. In some cases the nucleus occupies three-fourths of the cell, and is composed of an aggregation of numerous nucleoli, of about the 1–200th of a millimetre in diameter. At other times from one to four of these nucleoli may be seen scattered within the cell, either with or without a round or oval transparent nucleolated nucleus. On the addition of acetic acid, the cell wall is rendered very transparent, whilst the nucleoli are unaffected. Many of them are free, and at first look like altered blood corpuscles, from which they are at once distinguished by the action of acetic acid.

This power of increased development may be observed not only in the mesenteric, but in the spleen and other lymphatic glands. In a case of large epithelial ulcer of the leg, I examined the glands after death with great care, and found them much enlarged, dependent apparently on the excess of naked nuclei and increased number of cells they contained. I have observed the same alteration in the axillary and cervical glands. Its occurrence in the spleen has been previously noticed. This enlargement and softening of glandular organs is strictly analogous to what occurs in articular cartilages, from the increased development of cells, and a multiplication of nuclei within them, as observed and accurately figured by Dr. Redfern.

In certain morbid conditions, I have seen the ordinary epithelial or epidermic cells of an organ present the same tendency to multiplication. Thus, in the lung in certain cases of typhoid pneumonia, I have seen the epithelial cells exhibit the same multiplication, with increase of nuclei.

In the epidermic cancroid disease observed in the lips, and in the scrotum of chimney sweeps, the epidermic scales increase far beyond their normal size; the cells and nuclei also enlarge, and the latter often exhibit a disposition to multiply fissiparously.

In certain tumours of the mammæ, the ducts may be also observed to become distended with epithelial cells, constituting an increased growth of cell elements, which obstruct the tubes. A similar fact may be exemplified in tumours of the parotid, and in certain cases of cerebral meningitis affecting the ventricles, when the epithelium covering the choroid plexus is not only greatly increased in thickness, but many of the individual cells exhibit an increased number of nuclei, altogether distinct from fatty generation.

From all these facts, therefore, it is evident that, under certain conditions, the growth of cell elements in an organ, or on the surface of membranes, may be increased, and constitute diseases, the symptoms of which have been long known to medical men, although we are only commencing to understand their nature. In the lymphatic glands, as we have previously seen, these cells are frequently formed, and many of them enter the blood, and are visible there, constituting leucocythemia. An extensive inquiry is thus thrown open to the histological pathologist, having reference to the questions, how far do structural alterations in the lymphatic glands affect the blood? and how far do alterations of the blood re-operate upon the glands?

A peculiar alteration is occasionally observable in the spleen, which, it appears to me, can only be explained by the assistance of the facts previously detailed. I allude to the occurrence of an opaque discoloration and destruction of the glandular tissue, of greater or less extent, closely resembling a so-called deposit.

In many cases of leucocythemia, patches of this whitish matter were seen in the spleen; and in Case 11, the cut surface showed that the entire spleen presented this alteration in the various stages. A series of preparations in the University Museum exhibits this lesion in a variety of aspects, as I observed it during an epidemic of typhoid fever which occurred in this city during 1846-7. Occasionally the morbid mass softens round its circumference, and separates or sloughs out, when fatal peritonitis is the result.

On examining this altered texture in the spleen, with a power of 250 diameters

linear, it is found to consist of—1st, Numerous molecules and granules; 2nd, Free nuclei; 3rd, Compound granular cells of various sizes; 4th, Fragments of the fibrous tissue and fusiform corpuscles of the organ. The granular cells were frequently ruptured, more or less broken down, and appeared to me to be the remains of the large glandular cells formerly spoken of, which had undergone a disintegrating process. It is very possible that, under certain circumstances, the glandular cells enlarging in the manner previously described, cannot escape from the organ, and by aggregating together do not discharge their nuclei. They then undergo a disintegrating process, which constitutes the morbid alteration now alluded to.

A somewhat similar lesion, probably dependent on the same series of changes, is occasionally observable in the kidney and mesenteric glands. The alteration known as waxy liver is apparently allied to an analogous transformation. Here the tissue presents the appearance of yellow bees' wax; and the cells of the altered structure, instead of being filled with oil globules, as in the fatty liver, are empty, colourless, compressed together, and more or less collapsed and broken down. Hence there is not only a lesion dependent on excess of cell element, and multiplication of included nuclei, but its history may be traced by alterations in texture resulting from the disintegration of such cells.

The history of all these lesions has yet to be written. But the time, I trust, is not now far distant when a systematic pathology may be rendered possible, on the basis of chemico-histological research. Happily there are many men in this and foreign countries who are laboriously toiling to unravel the mysteries of altered structure, in connection with the clinical study of disease. To their investigations we must look as the only means of so advancing pathology, that it may be received by the profession as the true basis of a rational system of therapeutics.

In conclusion, I would direct the attention of my medical brethren to an extended study and investigation of the blood clinically. The means of doing this, with the assistance of the microscope, are now most easily attainable,* and the following points deserve attention:—

1. In some cases of typhoid fever, where there was every reason to expect disease of the intestinal glands, the colourless corpuscles have been stated to be increased in number. The importance of this observation will be recognised, when it is remembered, that, according to Brucke, these structures constitute the first series of the lymphatic glands. Further observations on an extended scale, however, are required, especially in France and Germany, where enteric fevers are so common.

2. An examination of the blood in cases of bronchocele seems advisable, especially in places where it is endemic.

3. In all cases where the blood glands are extensively affected, whether from hypertrophy, scrofula, or cancer, it is possible that the blood may be found more or less altered in the relation of the colourless to the coloured corpuscles. If so, further research may explain the contradictory statements which have been advanced, having reference to the actual detection of cancer cells in the blood.

4. In all cases of abdominal tumours, as a general rule, it may be well to examine the blood microscopically.

5. This should also be done in cases of purulent infection, puerperal fever, small-pox, glanders, syphilis, and all other disorders where the blood is affected on the one hand, and the lymphatic glandular system on the other.

6. A chemical analysis of the blood in these cases is desirable, as it is only by a multiplication of such observations that fair average results can be arrived at.

7. There are some cases of anemia which kill, apparently without obvious cause. In these it may be well to examine the lymphatic glands. In a boy who so died in Heriot's Hospital, under the care of Drs. Christison and Andrew Wood, the only lesion discovered after death was induration and cell disintegration of the mesenteric glands.

All this constitutes a series of researches, which can only be carried out by

* See the Author's Lectures on Clinical Medicine. Part V. Edinburgh: Sutherland and Knox. 1851.

interesting different individuals, especially those engaged in pathological investigation in large hospitals. Such, however, may be considered as absolutely necessary information to be obtained, before the laws of nutrition and the diseases connected with their derangement can be fully understood.—*Monthly Journal of Med. Science, April*, 1852, *p.* 331.

DISEASES OF THE NERVOUS SYSTEM.

12.—ON THE BLOOD ORIGIN OF A CERTAIN FORM OF GENERAL PALSY.

By Dr. Hamilton Kinglake, Physician to the Somerset and Taunton Hospital.

[It is only of late years that endeavours have been made to remove many of the affections of the nervous system from the class of organic diseases, and to include them amongst the "blood diseases." Much patient investigation will be yet required before we can connect the disease as a result of the elaboration of a morbid poison within the system. As an illustration, however, of this particular form of general palsy, the following case is worthy of careful perusal:—]

A clergyman, about forty-five years of age, residing in Devonshire, of a tall robust frame of body, and enjoying general good health, feeling somewhat indisposed on the 14th February, 1851, took, at bed-time, a calomel and colocynth pill. Early next morning, he imprudently stood for a quarter of an hour in his slippers and dressing-gown, superintending arrangements which were going on in a cold underground cellar. He was conscious at the time of becoming much chilled. It was soon perceived that his voice was hoarse, and he complained of sore throat, and *pain and stiffness of the calves of his legs*; the latter, being considered rheumatic, were treated with domestic remedies until the 20th instant, on which day medical advice was first taken, and he was discovered to be affected with paralysis. From increasing difficulty in walking, his gait had now become rambling; he felt his feet cold and numbed, also a slight tingling in his fingers: the tongue was clean, and there were no inflammatory symptoms present. The gentleman had come from a gouty family, and latterly had suffered one or two slight fits of the disease; his mode of living, a fondness for rich and piquant dishes, with the other attendant luxuries of the table, no doubt further rendered him prone to its attacks. He had, moreover, for some months past, been drinking an unusual quantity of cider, and living in a new house, which was, unavoidably during the winter, both cold and damp. He was constitutionally asthmatic, but suffered only in a slight degree, excepting under aggravating circumstances. It may further be remarked that, some years previously, he fell backwards in skaiting, and hurt his back, but not sufficiently to lay him up, although he felt it severely at the time.

A brisk aperient was given him; and after its operation, nux vomica in small doses. He was directed to bathe his feet in hot water every night.

By the 26th, motor-power in the lower extremities was almost entirely lost, and in the trunk and upper limbs greatly impaired; his fingers felt as though benumbed with cold; his voice was feeble; respiration at times laboured; bowels and bladder gave him a sensation of being distended, although both were evacuated. General sensation, and all the cerebral functions remained nearly, or quite, unimpaired; but his nights were very restless, and often disturbed by a dull pain in the region of the coccyx and tuberosities of the ilium, which was only relieved by his being posted up in bed. His tongue was thinly coated with a white fur; the gums free from lividity; throat sore and relaxed; he felt no thirst or feverish heat. The heart's sounds were faintly audible, without any attendant bruits; pulse small, weak, and intermitting every twentieth or thirtieth beat. He frequently felt faint. The urine was copious, sp. gr. 1012, clear, non-coagulable, and giving a strong acid re-action; a microscopic examination detected no oxalates or other crystals.

Mustard poultices were ordered to the ankles every night, and the following medicines:—

℞. Vin. colch. ℥ x.; liq. potassæ ℥ xx.; aq. distill. 3 vii.—4tis horis.

℞. Pil. hydrarg. gr. ij.; pulv. ipec. co. gr. iij.; ft. pil. Om. noct. sum.

March 1st.—Gouty symptoms appeared yesterday about his feet; and this day

both great-toe joints were red, swollen, and tender. The urine was highly loaded with lithates of ammonia. His pulse had somewhat improved; but in other respects he was much the same as on previous report. It being supposed the gout showed some tendency to recede, a hot pediluvium was ordered in the evening, after which, owing to unnecessary exposure to cold, he was seized with violent shiverings, which lasted three hours, and were succeeded by profuse perspiration. The gout entirely disappeared; he suffered from great faintness, cramps in the legs, and pulmonary congestion.

On the 4th, the paralysis was found to be much increased—the whole muscular system was enfeebled to the last degree, whilst all power in the inferior extremities was utterly lost; the sphincters were unaffected; but the abdominal muscles partook of the general loss of contractile power, thereby imparting the sense of fulness to the bowels (before alluded to), and rendering the process of defecation often a very tedious one. The trunk was feebly, and with difficulty supported, when the sitting posture was assumed; the diaphragm and muscles of respiration were working laboriously, and almost ineffectually, to detach by expectoration the viscid secretion of the bronchi. The hands, though totally unable to accomplish any precise movements, such as conveying food to the mouth, or buttoning clothes, were yet constantly grasping objects, with the view apparently of exciting the failing sense of muscular resistance. The power of swallowing was slightly impaired, as was also the articulation. The intellect and special senses were unaffected; the head was free from pain, or unnatural degree of heat. There had been no convulsion or spasmodic movements of the limbs; neither were any pains referred to the spine, or tenderness evinced on percussion of the vertebral column. There was a degree of restlessness in his manner, and an anxious expression of countenance, but he felt no pain. The heart's action had become more feeble, and fluttering; the pulse was soft, rapid, irregular, and intermitting every two or three beats; there was likewise an almost constant sense of faintness, which appeared to be increased by the ingestion of solid food, for which he expressed a frequent desire. The bowels were confined; the urine still high-coloured and turbid, and exhibited under the microscope numerous crystals of lithic acid.

He was desired to keep in bed (he having up to this time insisted on being dressed, and seated in a chair); and to have nutritious food in the form of jelly, beef-tea, &c., frequently given in small quantities, with such stimulants as might be requisite to obviate the existing prostration. Blisters were applied to the calves of his legs, and the following draught was prescribed:—

℞. Tr. cantharid. ♏ xx.; inf. cascarillæ ℥iss. Ft. haust. ter die sum.

5th.—Urgent cough and dyspnœa, with signs of subacute bronchitis and pneumonia, called for the substitution of salines and ipecacuan for the cantharides draught.

6th.—The pulmonary symptoms increased in urgency—a fetid mucus was with difficulty dislodged, in considerable quantities, from the air tubes, and impending suffocation obviated, by repeated doses of ether, and a large blister over the chest.

7th.—His condition remained most critical,—tongue dry and brown,—face flushed,—general temperature raised to fever heat, and pulse powerless.

Sumat 4is horis, Tinc. cantharid. ♏ xx. ex. aq. R. Hyd. chlorid. gr. ½. Pulv. ipec. gr. ij.; cons. q. s. ft. pil. 2dis horis sum.

9th.—Some general amendment evident. Mucous sub-crepitant râles confined to the left side of chest; the sputa, which were pinkish, and no longer fetid, dislodged more easily. He was only able to lie on the affected side; if turned, urgent dyspnœa and tracheal râles being the immediate consequence.

Cont. tr. canth. 6tis horis, et pil. 3tis horis. Admoveatur em. lyttæ inter scapulas dorso.

11th.—The chest complaints progressing favourably,—voice stronger, and expectoration more easy,—the circulation also recovering its balance,—pulse about 115, intermitting about every twentieth beat only. The bowels, which throughout had been torpid, were now constipated. A slight yellowness of the skin, which was first noticed yesterday, had now deepened into decided jaundice,—the urine was highly loaded with lithates, and deeply stained with bile,—tongue moist and cleaning. No mercurial action on gums.

Omittantur medicam. antea prescript. Habeat stat. enema tereb. et ol. ricin. et sumat decoct. aloes co. p. r. n. R. Hyd. chlor. gr. i.; ext. colch. acet. gr. ij.; ext. hyos. gr. ij. ft. pil. 4tis horis sum.

14th.—Gums slightly touched, and bowels freely purged. Evacuations dark, and contained bile, which they had not throughout the illness been defective in. Directed to omit the calomel pill, and take three grains of blue pill every night, to maintain slight mercurial action on the system.

28th.—The jaundice had entirely disappeared, and his countenance again wore its cheerful aspect. Although the bronchitis continued troublesome (requiring a persistance in expectorant and counter-irritant remedies), and great evacuation had taken place, the anticipated amendment, as respected the palsy, was beginning to show itself. After the muscles of respiration and those of the trunk—taking the inverse steps of their progressive loss of power—next in order of restoration were the hands and arms; and lastly, the legs and feet. His general health improved in equal ratio with the other amendment,—and by the aid of bitter ale and good nutriment, he soon regained his strength. The first week in April he was enabled to feed himself, and write his name again! By the end of April, to walk with assistance;—and by the middle of May, to wander unaided about his garden and grounds. The only other remedial measure, beyond regulating the stomach and bowels, which was adopted during the convalescence, was the application of the electro-galvanism to the limbs. Gentle mercurial action was maintained for several weeks, and repeated blisters were applied to the back, with the double object of relieving the pneumonic as well as the paralytic affection.

June 24th.—In a letter, received this day, the patient states that excepting constipation, and some tenderness and puffiness about his feet, he is better than before his attack of illness.

Remarks.—The case above detailed appears to derive its interest from the similarity it furnishes in its origin, progress, and termination to the class of diseases that owe their existence to the circulation of a morbid poison in the blood. It may be here remarked, with reference to the possibility of the disease being occasioned by the gradual introduction into the system of a mineral or narcotic poison, that the presence of lead (which was the only poison that, under the circumstances of the case, was at all likely to be absorbed into the blood *ab extra*), failed of being detected by the ordinary chemical tests, applied both to the cider and water that were habitually consumed by the gentleman and his family. The *new* house also in which they resided was *unpainted*, so that there remained no obvious source from which the lead-poison itself could have been furnished. Thus looking, in the first place, at the earlier stages of the malady, as represented by the premonitory feeling of malaise, the consciousness of being chilled, from the temporary exposure to cold, and immediately following upon that, the soreness of the throat, the aching of the limbs, and the general prostration of muscular power, we recognise the usual antecedents of an attack of epidemic catarrh. 2dly, The tendency exhibited by the disease to run a certain definite course, in spite of the remedial measures directed to its arrest or abatement; as also the fact of a relapse or general exacerbation intervening between the onset of the disease and its full development, are features in the case which more especially characterise the class of zymotic, as well as many of the so-called constitutional affections. 3dly, The limitation of the diseased action to one particular portion of a tissue (the motor tract of the spinal cord in the case in question) is not only imitated by the artificial introduction of poisons into the blood (witness the lead palsy, in which the poison fastens itself almost entirely on the motor nerves and muscles of the hand and fore-arm; the state of anæsthesia from the inhalation of chloroform; that of fantasia or ecstacy, from the administration of Indian hemp, alcohol, and other intoxicating substances, the one implying the localisation of the poison in those portions of the nervous structure that are chiefly subservient to sensation; the other, the fixation of the poison in such parts of the same tissue as are more particularly concerned in mental acts); but it is also illustrated by the natural course of various diseases, and has in fact its analogues in the local affections peculiar to the exanthemata, the iritis of syphilis, the affections of the fibrous tissues in gout and rheumatism, and in the localised eruptions proper to many of the skin diseases. Lastly, The gradual but progressive *amelioration of the palsy as* the fever and jaundice, with which it was complicated,

began to subside, and as critical evacuations through the kidney appeared to be obtained, are points in its history which assimilate it to the ordinary termination of those local affections that originate in, or are connected with, more or less of constitutional or febrile disorder.

Having thus cursorily viewed the case by the light of the humoral theory, it now remains to inquire how far the symptoms are capable of explanation, upon the assumed existence either of some structural change originating in the cord itself, or of some dynamic palsy bearing influence propagated thereto from the nervous extremities, distributed over the inner or outer surface of the body.

With respect to the former supposition, it may be remarked, that the entire freedom from all pain referred to the spine, coupled with the absence of any convulsive action of the limbs, appears sufficiently to exclude the idea of the loss of motor power being connected with any active congestion of the vessels of the cord or its investments; whilst the circumstance of the motor tract of the cord being alone affected, would seem equally to forbid the notion of the palsy being the effect of such mechanical pressure as would result from hemorrhage or serous effusion into the spinal canal, seeing that the pressure of a fluid could hardly be exercised upon one division of the end (the motor) throughout its whole extent, without affecting, to a certain degree, the other also.

The hypothesis of the peripheric origin of the disease, would seem to fail, not only by reason of the absence of the causes deemed essential to its development, such as the prolonged exposure of the extremities to excessive cold, or continued irritation of the mucous surfaces; but also from the circumstance, in the case under consideration, of the very rapid extension of the morbid change to all parts of the motor column; whereas, in peripheric paraplegia going on to general palsy, the disease creeps on by slow and almost imperceptible advances, and scarcely ever perhaps reaches so far up the spine as to implicate the nerves supplying the organs within the chest and superior extremities, without being complicated with such an abnormal and pre-existing condition of the cord, as would tend, with the additional or exciting cause referred to, to give an irremediable character to the disease.

Seeing, therefore, at how many points the case recorded touches one or other of the morbid states belonging to the great class of blood diseases, and how insufficient any other hypothesis is, for the true expression of the peculiar symptoms disclosed in the progress of the malady, one can hardly avoid including it in the group to which it appears to be most closely allied.

Any elaborate attempt to assign a more exact pathology for the disease in question, by seeking to determine the specific nature of the assumed blood poison, and the conditions which appear to have rendered certain portions only of the nervous structure obnoxious to its agency, is necessarily precluded by the prescribed limits of this paper.

It may be briefly remarked, however, with the view of indicating the direction in which the "materies morbi" of this and similar diseases may probably be found, that, with the exception of the carbonic acid, *urea* is the only excrementitious matter naturally existing in the blood that is certainly known to act (when accumulated in the system to a sufficient amount) as a narcotic poison on the nervous structure.

Although the colouring matter of the bile is also held to possess a narcotic property, it appears somewhat doubtful whether this principle, acting *per se*, is sufficient to fully narcotise the nervous system; seeing that the cases of jaundice associated with nervous symptoms, and terminating in coma, are generally the result of a *suppression* of the biliary secretion from mental causes, which, it is to be presumed (acting more or less generally as they do), would extend their paralysing influence to the other secreting organs of the body, including more particularly the kidney, while the blood would become further vitiated by the poison of urea—thus acquiring the necessary amount of narcotic impregnation. With respect, also, to the carbonic acid naturally existing in the blood, it may be remarked, that although its undue accumulation therein from deficient action of the respiratory organs, is calculated to act prejudicially upon the system, it would fail to exercise its strictly *narcotic* influence, until the blood became so far saturated with the gas as to induce the state of asphyxia, of which there was no indication in the case under review.

Now, applying this fact to the disease under consideration, and bearing in mind that there is direct evidence in the case as recorded, of there being formed within the system a larger quantity of bile out of one portion of the unassimilable matter contained in the blood, viz.,—the carbonaceous,—than could be excreted by the liver, may it not be presumed, looking at the gouty diathesis presented by this individual, his habit of high feeding, and the symptoms of gout which actually declared themselves in the course of the disease, that the system had, in like manner, been previously so saturated with the other division of waste matter, viz.,—the nitrogenous,—as to have elaborated therefrom a larger amount of the urine elements than could be immediately excreted by the kidneys; and that the surplus urea so retained in the blood was expended in narcotising such parts of the mucous tissue as became subjected to its agency? The specific gravity of the urine (1012) at the earlier stage of the malady, coupled with the fact of no lithic acid crystals being detected by the microscope, would render it probable, that not even the ordinary proportion of its solid constituents was eliminated by the kidneys at this period. Later in the disease, and contemporaneous with the appearance of gout in the extremities, there was an abundant excretion of the lithates; and this, I take it, was the first attempt on the part of nature towards a restoration to the healthy state; whilst the relapse may be certainly dated from the period of intense febrile movement that subsequently occurred, and which had the effect both of removing all trace of gout in the parts previously affected, and of throwing upon the motor tract of the cord the poison that the system had before endeavoured unsuccessfully to rid itself of.

The localisation of the assumed urea poison in the motor tract of the cord, instead of in the brain, to which, in ordinary cases, it would appear to be principally attracted, might be held to exemplify that desertion, or shifting of a morbid poison from its natural habitat to a weaker and less resisting portion of the same, or even a different tissue, which is illustrated so frequently by the pericarditis supervening on rheumatism, the erysipelatous forms of peritonitis, the various internal disorders consequent on the sudden "striking in" of certain cutaneous eruptions, and more especially by the erratic course and uncertain seat of spurious or undeveloped gout. A further illustration of the occasional localisation of a poison in other than its ordinary seat, is afforded by a circumstance communicated to me by a gentleman, who had lately undergone a severe surgical operation under chloroform. His statement was this,—that having inhaled what was considered a full dose of the chloroform, he became perfectly paralysed to all motion; but yet retained a perfect consciousness of everything that was passing around him, and, moreover, felt the pain of the operation in its full intensity, thus showing that the poison had fastened on the motor rather than on the sentient division of the nervous structure. It is not impossible but that the severe fall on the back, which the subject of the foregoing case encountered some years since, may have disposed the spine to become the seat of the assumed poison rather than the brain, and thus have been the means of saving him from, perhaps, a fatal attack of what would have been termed nervous or simple apoplexy.

The question as to whether the hypothesis that has been thus provisionally framed to meet the case above detailed, is applicable or not to certain of the so-called functional diseases of the nervous system, such as the partial or complete anæsthesia of trance, catalepsy, hysteric coma, and other morbid states of a like nature, characterised by a sudden, though temporary abolition of sentient or motor power, is well worthy of considerate investigation, seeing that they present, in their limited, and often periodic existence, in their strong tendency to terminate in a complete restoration to the healthy state, and the absence in fatal cases of all visible lesions of structure, the marks which essentially distinguish that other branch of the same family of nervous diseases, in which a perversion rather than suspension of functional power exists, such as neuralgia, epilepsy, chorea, delirium tremens, puerperal mania, and perhaps, also, hypochondriasis, which appear in many instances obviously to result, either from the introduction of some morbific element into the blood, or from the elaboration within the system of certain noxious matters of an irritant character,—which failing, it may be presumed, of being eliminated by the excreting organs, or of being temporarily withdrawn from the circulation, through *their natural attractiveness* for, and incorporation with, the cutaneous or other com-

paratively unimportant tissue of the body, fasten themselves upon certain parts or divisions of the nervous apparatus in obedience to the affinity for the poison matter, created in such parts through the lowering and exhausting influence to which they may have been previously subjected.

It may be remarked in conclusion that the urine of hysterical subjects, in whom many of the diseased states above enumerated chiefly occur, is often notably deficient in solid constituents, which can only be ascribed to a temporary suspension of functional power in the kidneys, to separate from the blood the peculiar excreta destined to be eliminated through that channel. This partial and modified form of ischuria renalis is occasioned, for the most part, by sudden mental emotion, which acting, it may be presumed, upon the chemistry of the living body, after the manner of the electric force upon that of certain inorganic matters, effects, among other changes, the temporary suspension of those normal affinities or relations that subsist between the excreta retained in the blood, and the organs to which they are specially attracted, for the purpose of being removed from the system.

It is not improbable, also, that the same cause which, acting through the nervous system, thus incapacitates the excretory organs from duly performing their functions, is also instrumental in so lowering the vital energies of certain portions of the nervous substance, as to render such parts peculiarly obnoxious to one or other of the excreta so retained in the blood,—that is, either to the urea or narcotic element, or to the more irritating ingredients, not only of the urine (as represented perhaps by the lithic acid and its salts), but also of the sweat, and menstrual and intestinal secretions. In the former case, the resulting diseases would be marked by more or less of suspension of the sentient or motor power in the part of the nervous apparatus so affected, whilst in the latter event the group of nervous affections that might ensue would be characterised by an exaltation or perversion of functional power, limited to the part in which the assumed irritant poison might happen to locate itself.—*Monthly Journal of Med. Science, Feb.*, 1851, *p.* 101.

13.—CASES OF RHEUMATIC AND LOCAL PARALYSIS, TREATED BY PULVERMACHER'S CHAIN BATTERY.

By T. J. Vallance, Jun., Esq.

[Mr. Vallance highly recommends this chain battery in preference to the same strength of currents from the ordinary machine, believing that the current from the hydro-electric chain of Pulvermacher has a much greater effect. The following cases are given as illustrative of its action:]

Case I.—Alfred Cooke, aged 26, a gardener, was attacked on the 4th of July, 1851, with acute rheumatic fever. He was treated in the usual manner, and was confined to his bed about a fortnight, suffering much in the muscles of his extremities and back. After he was able to leave his bed, he had great pain in his shoulders, wrists, and loins, increased by the slightest motion, so that he was incapable of dressing or feeding himself. He continued in this condition up to the 29th of August, (nearly nine weeks,) during which period he was treated by tonics, occasional doses of Dover's powder, and turpentine frictions; these, however, had no apparent result.

On the 29th I found his arms hanging useless by his sides; the right was somewhat the worst; it felt cold, the skin tense and hard, and the muscles much atrophied; the hand he was unable to close. The metacarpal joint of the index-finger was much swollen; it felt hard, and was very painful on pressure, or on any attempt to bend it. Thinking this a fair case for the application of galvanism, I resolved to try the chain battery, and accordingly passed a current from one hand to the other. The first effect produced was considerable faintness; the man, however, quickly rallied, and ten minutes having elapsed, I found he could close his hand perfectly, that the swelling was considerably diminished, and that all pain was gone. The same evening he undressed himself without assistance.

Aug. 30th.—Arms slightly painful on motion. The current was again passed with marked benefit.

Aug. 31st.—A good deal of pain in hip and knee, increased by movement.

Current was passed in the direction of the sciatic nerve, which removed the pain.

Sept. 3rd.—Slight returns of pain, and immobility in the upper extremities, speedily removed by the passage of a current.

From this date to the 10th, as there remained some trifling stiffness in the limbs, a moderate current was passed every morning. Its use was generally followed by a little diaphoresis, and a slight feeling of exhilaration.

On the 11th the man returned to his work.

Case II.—Edward Connor, aged 60, a labourer, strong habit of body, has been prevented following his employment by rheumatism for three months; first seen by me August 30th, when he complained of violent pain in the shoulders and neck, which was so much affected that he was unable to turn his head; he complained also of great pain in the right clavicle, on examining which I found a considerable amount of periosteal induration, forming a large swelling near the sternal extremity. I pointed this out to my friend Mr. Beale, of Plaistow, and then, assisted by him, passed a current along the clavicle for about five minutes, during which time the man said he felt the pain leaving him, and, at the end of that period, that the pain was gone. On re-examining the part, the swelling had disappeared, but the man is still unable to lift his hands to his head.

August 31st.—Finding much induration of the subcutaneous cellular tissue over the pectoralis major, I passed the current in the direction of the muscle, producing powerful and rapid contractions. I then passed the current from hand to hand for about ten minutes, when it caused a gentle diaphoresis, and expelled the pain from the shoulder, and enabled the hands to be elevated above the head without much difficulty.

Sept. 1st.—Neck very stiff and immovable, but not painful; the current was passed through it for a quarter of an hour, after which it became capable of motion.

Sept. 2nd.—Current again passed through the neck, attended with good result.

Sept. 5th.—So much improvement has taken place, that the man says he shall go to work next week.

Case III.—30th Sept., 1851, Sarah Whittaker, aged 70, states that in the month of February, 1851, she fell down a flight of stairs, fractured two ribs, and sprained her back, and was confined to her bed for seven or eight weeks afterwards; but never entirely recovered, having almost lost the use of her right shoulder and neck, so that she is unable to turn her head; has suffered frequently from rheumatism. On examination, I found her very weak and thin, and the muscles of the right shoulder much smaller than those of the opposite side; there was no difference, however, in temperature or sensibility to the touch.

I passed an intermittent current through the back of the neck, which relieved the pain, and enabled her to turn her head; I afterwards passed the current from hand to hand for half an hour; it did not, however, produce so powerful an effect on her as it does on most persons; this is easily accounted for by her age, as I uniformly find young persons more susceptible to the influence of galvanism than are the old; it however considerably improved the power of motion in the shoulder.

Oct. 3rd.—Battery again used with good effect.

4th.—The current repeated.

5th.—Better; repeat the current.

7th.—Much better; can now lift her hand to the back of her head, and feels better than she has done since her accident; the current was again applied, and motion improved.

9th.—Said she had nothing to complain of.

N.B. When the word current occurs in the above cases, it is to be understood that in each instance the intermittent current was employed, produced either by the interrupting cylinder or clockwork.—*Med. Times, Nov.* 15, 1851, *p.* 509.

14.—*Paralysis treated by Ergot of Rye.* The following are three instances, related by M. GERARD, physician to the Hotel Dieu at Marseilles, in support of the efficacy of ergot of rye in doses of from half a gramme (7¾ grains) to two-and-a-half grammes (38½ grains) in paralysis of the lower limbs.

Case I.—A miner, aged 39, of strong constitution, after having worked for some time in a damp situation, was attacked with paraplegia. The paralysis was complete during eighteen months, being accompanied with incontinence of urine; but it then diminished so far as to enable the patient to walk a few steps with the aid of a stick, although with dragging of the limbs, and irregular gait. The affection had lasted four years, and the treatment had been various. On Jan. 2nd, half a gramme of ergot was administered, and the dose was daily increased two-and-a-half grammes. In the course of Feb. there was marked improvement. On March 15th, the patient walked to the bath without aid. Towards the end of April, the incontinence of urine had diminished, and on May 31st, he was dismissed, cured.

Case II.—A man, aged 29, addicted to spirituous liquors, had trembling, with incomplete paralysis and anæsthesia of the lower limbs. He recovered under the same treatment in two months.

Case III.—A man, aged 23, had been exposed in Algeria to damp during four days; this was followed by fever and delirium which lasted for a fortnight. During convalescence, he found that he had paralysis of the right arm; when the limb recovered, the right leg became affected, and then the left. He had paralysis, with incomplete loss of cutaneous sensibility, in both thighs. After various modes of treatment had been tried in vain for three months, ergot was given in the same doses as above mentioned. In twenty-nine days there was great improvement, and the cure was complete in three months.—*Bull. Gén. de Thérap.—London Journal of Medicine, Jan.* 1852, *p.* 89.

15.—ON PERIOSTEAL DISEASE AFFECTING THE DURA MATER.

By DR. R. H. GOOLDEN, Assistant Physician to St. Thomas's Hospital.

[In previous notices of this disease, Dr. Goolden has shown some important, but not generally recognised forms of its development, and its independence of syphilis. In considering its remedial treatment, he says]

No more valuable remedy has been introduced into medicine than the iodide of potassa, but its precise value is scarcely generally recognised. Some practitioners give it in eight-grain doses, three times a day; and give it with calomel or blue-pill, and colchicum; others never venture beyond two grains, and even then fear the results; yet no one ventures to dispute its almost general efficacy in nodal periostitis. The very large doses that were exhibited by Dr. Elliotson, in the year 1832 (see 'The Lancet'), must be regarded as one of the many valuable experiments for which we are indebted to that enterprising investigator, but which show rather what quantity may be given, under certain circumstances, without mischief, than what is the appropriate quantity for a dose, in order to obtain its specific effect. As far as I can draw any inference from my own cases, I should say that smaller doses produce the specific effect quite as soon as the larger; and I have often known two-grain doses act readily on the system, when larger doses have failed; but I think it necessary to combine the salt with liquor potassæ, as any acid on the stomach will decompose the salt, and the free iodide will thus be very likely to act as an irritant. Indeed I have not unfrequently found, that patients who had been trying the iodide of potassa, and obliged to discontinue it, have borne it very well in combination with the fixed alkali. I may also observe, that it is not well borne where there is any considerable gastric irritation, indicated by a foul tongue, dry skin, præcordial pains after food, &c., attended with acid eructations, and especially where there is urine turbid with lithates or lithic acid; and yet we may have all these, as well as periosteal nodes, to contend with; and I am quite sure, that during the active forms of gout and rheumatism, the medicine is never well borne; yet it is often very useful in relieving pains which are still existing after all constitutional traces of specific disease have subsided. It is therefore necessary to relieve the dyspeptic symptoms by appropriate remedies,

before using the iodide of potassa; or not to set down our failure to the inefficacy of the salt. The disagreement of the medicine is soon detected by the excessively nauseous, metallic taste in the mouth, before the gums are affected, or the catarrhal symptoms so severe as to oblige us to discontinue it; and when that taste distressingly prevails, the salt rarely produces any beneficial effect.

But I will even go further in asserting, that in some instances periosteal nodes have been the result of the very medicine we are giving for their cure. Of this I can produce some instances, but one in particular deserves our reference, as occurring in the person of a medical practitioner, a man of considerable talent and great reputation in one of our principal provincial cities. He had been overfatigued by a number of midwifery cases occurring in succession, which had interfered with his nightly rest, and a scrofulous bubo appeared in the groin, which suppurated. He had not had any syphilitic affection, but he took the iodide of potassa in five-grain doses three times a day, under which medicine the matter was absorbed, but the whole time that he was taking the medicine, he was distressed with a constant metallic taste, that flavoured all his food, and continued for eight days after he had discontinued the medicine; but what induced him to discontinue it, was the appearance of excessively painful periosteal nodes on the tibia and elsewhere, and which were relieved by mercury as soon as the mouth was sore. He is now in good health.

As long ago as the year 1812, Mr. Crampton, in the first volume of the 'Dublin Hospital Reports,' recognised a form of periostitis unconnected with syphilis, and referred to cases in which the dura mater was its seat. There will there be found several cases where relief was afforded by blue-pill, taken so as to affect the mouth; but recourse was likewise had to the painful operation of dividing the periosteum. These cases are valuable as containing the autopsies of several cases which were fatal previous to the introduction of the iodide of potassium, and as showing the state of practice at that time. It should be observed that success did not usually attend this treatment. I have met with cases of periosteal nodes, which have appeared during a course of iodide of potassium and sarsaparilla, taken for secondary skin eruptions, and which have subsided on substituting mercurial medicines for the iodide; in the following case, quite unconnected with syphilis, relief was attendant upon salivation where the iodide of potassa had failed.

Mary B——— was admitted into St. Thomas's Hospital, under my care, complaining of very severe headache, constant, but aggravated by paroxysms every night; the brows were corrugated, and the expression of the countenance indicated cerebral oppression. She had been ill for eighteen months, and her sufferings had been gradually increasing. She was twenty-seven years old, and single. She had been losing flesh since her illness; no great amount of perspiration, but still the skin was moist; pulse 120; white furred tongue, and much thirst; catamenia suppressed; no indications of tubercle in the chest on auscultation. She had been taking five grains of iodide of potassium three times a day for a week, but without any benefit, and complained of a metallic taste. Bowels open; no appetite. The medicine was discontinued, and a dose of castor oil given, and she was ordered a draught consisting of ten grains of nitre, half a drachm of liquor potassæ, half a drachm of spirits of nitre, and an ounce and a half of camphor mixture, three times a day, and a blister to be applied to the crown of the head. She thought herself slightly relieved when the blister had drawn, but the pain is now as bad as ever; the blister nearly well; pulse still 120; but the tongue clean, and less thirst. She is ordered to take, in addition to the medicine, two grains of mercury with chalk thrice a day, and the blister repeated. No opium was given on account of the state of the tongue. The gums are touched with the mercury, the blister still open. She states that as soon as the mouth became sore, the pains in the head nearly left her; the tongue much swollen and white, with a brown streak in the centre; pulse 100; and although suffering all the inconvenience of salivation, expresses herself as most thankful for the relief. She is to omit the mercury. I have extracted enough of this case to illustrate the point that mercury may control the disease when the iodide of potassa fails; but I may go on to state, the headache returned, though with much less intensity, but is gradually subsiding under steel and quinine, with a full diet and wine. The tongue *is clean, and pulse 80.* I should mention that I lately tried two-grain doses of

iodide of potassa with liquor potassæ, but it evidently disagreed with her, and was discontinued. The patient is still in Mary's ward.

A few months ago I was consulted by Dr. Purcell, of Newington, in the case of a lady suffering from nightly paroxysms of pain in the head, partial paralysis, and well-marked indications of periosteal disease in other situations, as about the bones of the pelvis and long bones. During treatment she had suffered most severely from salivation after the exhibition of a very moderate quantity of mercury; but that had long subsided, and I ventured to recommend doses of iodide of potassa, with liquor potassæ, and compound decoction of sarsaparilla. This again produced a most severe salivation, with no relief to the symptoms. These cases are very rare indeed; but I should observe that the patient was advanced in years, and with a constitution that had been much shattered by domestic troubles.

Under these circumstances, it remains to invigorate the system by nutritious diet, taking care not to give the digestive organs more to do than they can easily accomplish, and to exhibit such tonics as in the individual case seem to be most suitable. It will be found that the different preparations of iron with quinine, sometimes gentian and ammonia, sometimes nitric acid, and decoction of sarsaparilla, may be given with advantage; but the relief is very different in degree and kind from that produced by the iodide of potassa or by mercury in the cases to which they are respectively applicable. I have occasionally observed some benefit to accrue from the use of the phosphate of ammonia, where the iodide of potassa could not be persevered in. I must first refer to iron as a remedial agent. I have not lately used iron in any form as a remedy until satisfied that the patient was intolerant of iodide of potassa, or else as a tonic during such intervals as it has been found desirable to intermit the use of that salt; but amongst those who have applied to me as private patients, I have been able to collect facts relative to the effects of iron which had been previously reccommended.

Of all remedies, next to the iodide of potassa, iron has certainly been generally the most serviceable; but the form in which it is exhibited does not appear to me at least to be of much consequence. A lady, near Dorking, consulted me for neuralgic pains on the right side of the face, from which she had suffered for six years. She had at different times consulted some of our most eminent physicians and surgeons, and derived some benefit from carbonate of iron, prescribed for her by Sir B. Brodie; but it never quite relieved her. The tic occurred in paroxysms in the evening; and on questioning her, I learned that she had also headache, pains in the crest of the ilium, and tibial nodes. She had the appearance of one who had been much stouter, but was emaciating and anæmic. Small feeble pulse; clean tongue, and had no sleep for pain. She took iodide of potassium, five grains; liquor potassæ, half a drachm; compound decoction of sarsaparilla, an ounce and a half; thrice a day. An opium pill at night.

Under this treatment she gradually lost all pain. The opium was soon discontinued, but she slept well, and the medicine was at length omitted, in consequence of the disagreeable metallic taste. Her medical attendant, Mr. Curtis, favoured me with a favourable report of her since, and she is now taking quinine and iron under his direction.

The carbonate of iron was considered to have a kind of specific action in such neuralgic cases, and was prescribed in spoonfuls; but I think it a clumsy way of giving medicine; being nearly an insoluble powder, and any quantity of course may be given that will pass off by the bowels. The more soluble salts are certainly the most effective, and none more so, or more agreeable, than the vinum ferri as prepared at Apothecaries' Hall; but the sulphate or hydrochlorate, or the ammoniated tincture, will answer equally well. The chalybeate waters are even more certain in their effects than any of our medicinal potions, and this is a fact not so generally recognised as it should be. It matters not whether the water be natural or artificial. Small doses largely diluted are more readily absorbed and diffused than larger and more concentrated doses; and I am sure the experience of all practitioners who have had the opportunity of observing it, will accord with my own in this respect, that some persons who have taken the pharmaceutical preparations with little or no effect, have been unable to persevere in the use of the natural ferruginous waters, from their immediately affecting the head.

The anæmic appearance usually observed in periosteal disease would seem to

point naturally to iron preparations, and though they do not seem directly to control the morbid action going on during the activity of nodal deposit, as both iodide of potassa and mercury, in their appropriate cases, are observed to do, yet their indirect influence is very decided, and when the acute symptoms are relieved, it is most desirable to alternate the iodide of potassa with iron preparations; and there is one form of iron which is especially applicable, viz. the iodide, which is best given in the form of syrup.

Quinine is an important agent in the treatment of this disease. Periosteal disease is rife in the aguish districts of Kent and Essex, and although the iodide of potassa will control the paroxysms, the relief must be followed up by such remedies as restore the constitution to a normal state, which the iodide will not do, and without which the relief can only be temporary. There is a morbid condition popularly recognised in those situations under the term "dumb ague." No distinct paroxysm of ague is observed; but it is evidently dependent upon the same influences which may develope fever in its intermittent or remittent form; attended with much debility; sense of chilliness, emaciation, and anæmia; and its victims are constantly the subject of periosteal disease. The women cease to menstruate, and the men are despondent; the anasarcous swellings indicate an obstruction in the chylopoietic vessels, whether in the liver, spleen, or kidneys, and Bright's disease is no unfrequent attendant, evinced by albuminous urine. Without hoping to restore the kidney to its normal function when that is evidently degenerated, much good accrued from the exhibition of quinine, either alone, or in combination with sulphate of iron, dissolved in dilute sulphuric acid.

And here I hope I may be allowed to interrupt the order of my paper to refer to the effect of large doses of the disulphate of quinine in the treatment of the common continued fever of this country. It was first pointed out to me two years ago by Dr. Leslie, an eminent physician at Rio, who prescribed it in doses of ten grains every two hours in the treatment of the remittent fever of that climate. An opportunity was afforded him of trying it on a large scale during his late visit to this country and Ireland, and from his reports, unpublished, I was induced to try it. At the same time it was tested at the Northern Hospital at Liverpool, by my friend, Dr. Dundas, formerly of Bahia, whose great experience in the treatment of disease in various climates, and sound philosophic caution, render his testimony of great value; and I hope shortly, with the editor's indulgence, to give the readers of this periodical a more fully detailed statement of my own clinical observations upon the subject, for there is much to be said concerning it that would lead me now too far from my subject. It is sufficient for my present purpose, that I mention that the effect of the quinine in fever, exhibited in frequent and large doses, is perfectly safe. It equalizes the circulation, relieves the visceral congestions, checks the diarrhœa, and whether the form of fever be the severe synochus or the low typhoid, the results are equally satisfactory and decided.

But the object I have in view is to show, that in the treatment of disease something more is to be attributed to quinine than a mere tonic influence, and I believe it to be a most important agent in restoring certain morbid conditions of the constitution to such a normal state that the organism may become less obnoxious to specific forms of disease, amongst which the periosteal is prominent. Where a simple tonic is required, quinine is not the most efficacious, and is often observed rather to oppress than invigorate the digestive functions, (I am acquainted with two persons in whom severe inflammation of cellular tissue is the result of a grain dose of quinine;) but some of the simple bitters, with ammonia, are often found more useful. This holds good mostly in tipplers—I do not mean absolute drunkards, but those who are constantly drinking small quantities of wine, dilute spirits, or even beer, at all times and seasons, and in cases where constitutions have been debilitated by residence in the tropics, or by the exhausting passions. I have not been able to satisfy myself that there is any reason for preferring one bitter rather than another. I have prescribed gentian, quassia, cascarilla, cusparia, and simarouba, each for a stated time, in order to watch their comparative effects, and I am inclined to think that any preference is rather a matter of fashion than founded on any carefully and extensively watched clinical observation; but taking our Pharmacopœia preparations, I think I may safely say that *the dose should* never exceed half an ounce in an ounce and a half draught.

Concerning topical applications: I have already referred to the efficacy of issues and blisters, and in the severer forms of cephalalgia arising from the disease of the dura mater, I prefer them; but I may also recommend the application of spirit of wine, the most convenient mode of using which, is, to wet some lint with the spirit, and having applied it over the head or other painful part, to prevent evaporation by covering it over with oil-silk: it produces a copious sweat in the part, and with very great relief. I need hardly mention that eau de Cologne, or any strong spirit most readily procurable, will answer the purpose. Old nodes may be painted with the tincture of iodine, or rubbed with the iodine ointment, but these are not convenient applications to the scalp, and the latter is objectionable where there is much tenderness.

There is only one more remedy which I must not omit to mention—that is, guaiacum. It is by no means universally applicable, but in some cases most decidedly useful, and especially where there is a cold dry surface, and languid pulse, and where the iodide too readily produces catarrh. Lest I should confound such cases with the catarrhal rheumatism affecting the head or other parts, I have been cautious to draw my inferences only from cases in which tibial or other easily recognised nodes are seen and felt. It is still possible that frequent catarrhal affections of the mucous surfaces of the nose and forehead, together with their depressing influence, may not only determine the neighbourhood of periosteal nodes, but prevent them from readily yielding to specific remedies; and it may be on that account that the guaiacum is found so serviceable in particular cases, for it is a practical fact, not half so generally known as it deserves to be, that the common cold in the head, however severe, may be at once relieved by guaiacum, These are cases in which a physician is not often consulted; but in my own person, and in my family, and the circle of my intimate acquaintance, I have for a long time had recourse to the remedy with invariable success.—*Lancet, Dec.* 20, 1851, *p.* 575.

16.—NOTES ON CASES OF SYPHILITIC MENINGITIS.

By Dr. Thomas Read, Belfast.

[Amongst the most remote and latest, but exceedingly important signs of constitutional syphilis, are those of a cerebral origin. Little notice, however, appears hitherto to have been paid to this fact. M. Ricord, speaking of "the action of the osseous affections on neighbouring parts," says, that one consequence of this species of compression is epilepsy, the fits seizing the patient as the osseous growth gets more considerable and irritating. He also mentions paraplegia as a casual effect of tertiary syphilis in the bones. Mr. Acton merely quotes this extract from M. Ricord, but upon the *disorder of the mind* they are both silent.]

Case I.—*Syphilitic Meningitis; Paraplegia; Hemiplegia: Amaurosis; Difficulty of Articulation, with Mental Hebetude.*—Mr. F., in July, 1847, was accompanied to my house by a surgeon of Belfast, who was naturally alarmed by the rapid advances of paralytic and other cerebral symptoms in his case. The patient required to be supported on his feet while his clothes were removed from the upper part of his person. His speech, from difficulty in articulation, was very imperfect; his powers of arranging his ideas and memory were slow and defective; vision very imperfect in both eyes. The gentleman who accompanied him had known him for some time. He had been treated for years by another surgeon for secondary syphilis,—chiefly intractable ulcers on the limbs, face, and head; and on these parts there were several large cicatrices. He had placed himself under the former gentleman's care for progressively increasing amaurosis; the paralytic symptoms supervened, and rapidly advanced until he exhibited the aggravated and almost hopeless state of general paralysis in which I saw him. The history of the case, his countenance, and general appearance, led me to the impression that all the symptoms might be assigned to compression of the brain by development of syphilitic tumours of the dura mater; but as this could not be a certain diagnosis, and to guard against an error in judgment, I considered the rapid exhibition of mercury to be the safest course. I therefore advised the entire scalp to be shaved, a blister applied immediately over it, and one drachm of strong mercurial ointment

to be rubbed in on any convenient surface twice a day. But I apprehended a speedy appearance of coma, and a necessarily fatal result.

Some eight days afterwards I saw his surgeon, who told me that our patient had made a rapid recovery, and was then able to run up and down the steps of a very steep staircase. To my additional surprise, a well-looking, dark man, with clear and expressive eyes, addressed me one day in the street, and, asking me if I did not know him, brought to my recollection the visit of himself and his medical attendant to my house less than three weeks before. He was completely recovered, looked in full health, had perfectly regained his powers of motion, vision, and articulation, and was in full possession of all his faculties. Mercury here released every oppressed organ, gave flesh and strength by restoring the animal functions, and afforded one of the happiest triumphs of medical art over disease I ever witnessed. Death must rapidly have closed the scene, had not this powerful and efficient agent arrested its course.

Remarks.—This patient was about thirty-two years old, with eyes, hair, and complexion as dark as any native Italian; well formed, naturally of a very robust frame, and with the semblance of a good constitution. About four years before I saw him, after a suspicious intercourse, a bubo formed in the groin. No sore or cicatrix was seen on the penis. He placed himself under a well-educated and skilful surgeon, who thought it advisable, notwithstanding the absence of chancre, to administer mercury for some time, under the use of which the bubo disappeared. After he had discontinued mercury, it again formed, and burst while he was engaged in active exercise. Some months subsequently he was attacked with an eruption which produced intractable ulcers. His surgeon treated him without success, as to their healing, for many months, until he went to the sea, and then they healed while he was using the open sea-bath.

It is worthy of observation, that here we have a bubo without chancre. This bubo was certainly not phagedenic, yet was followed by what I conjecture to have been rupia.

Whether a man be a mercurialist or a non-mercurialist, he may admit it to be quite true that the ulcerative forms of syphilis do not encourage the employment of mercury; but I feared to rely on hydriodate of potash in a case of such extreme emergency. And I must add that I still have to acquire the enthusiastic confidence of M. Ricord in its unerring dominion over tertiary syphilis.

The patient has since been well able to pursue his business in life, but has suffered many of what M. Ricord calls the accidents of tertiary syphilis; he has had many attacks of cranial pain, want of sleep, pain, enlargement of bones and joints, and very lately he had double sarcocele; all have, more or less readily, yielded to the hydriodate of potash. The doses are now raised to half a drachm three times daily. He has the confirmed syphilitic diathesis; but he makes out life '*versus*' disease, with good grounds to expect his constitution will finally surmount his malady; his life has never, since the paralysis, been in the slightest danger.

Case II. *Syphilitic Meningitis; Mental Incompetence; Incoherency; Suicidal Propensity; Paralysis.*—Mr. M., a gentleman about twenty-six years of age, was attacked with hemiplegia of the right side while travelling in a night mail. His intellect was disordered, and he was incapable of appreciating his own state and directing accordingly. A clergyman, who lodged in the same house, requested me to see this gentleman about eighteen hours after the attack. He informed me, that some months before he had observed incomprehensible ramblings in his conversation, and the expression of strange and incongruous ideas. This had become apparent in the course of his daily occupation, one of trust and considerable mental labour; and the head of the establishment had placed him under the care of his own physician, a gentleman of experience and in extensive practice, who considered it requisite to have *every means for self-destruction removed out of his reach.* After some length of treatment, he advised change of scene, and all the adventitious aid of new associations and amusements, if possible to change the current of his ideas. He went to friends in London, and while there, they deemed it requisite to induce him to place himself under the care of a physician, who (on what ground I do not know) prescribed for him hydriodate of potash, with decoction of sarsaparilla, which he used for about five weeks. He returned, in some measure improved, and *resumed his official* duties. He was on business connected with his office, and

returning home, when he was seized with paralysis. All this I learned from the clergyman, whom motives of humanity induced to take a great interest in the matter. He was aware that the invalid had formerly been under my care, and that I probably had a more intimate knowledge of the private history of his life than any one else. In fact, he had come to me with an excavated syphilitic ulcer of the throat more than twelve months before. I was dissatisfied with his want of steadiness in following my directions, and he broke off attendance before I dismissed him. In some months after he again called on me for advice. He had syphilitic iritis of both eyes. There was no eruption. He again returned to his employment, earlier than I advised. Considering the antecedents, and the slow progress of his cerebral symptoms, which I count as significant of intra-cranial syphilitic disease, I believed that his disease was syphilitic meningitis, with deposition. I took blood from the arm, had the head shaved and blistered, gave active purgatives with tartar emetic, and, after the free action of the bowels, began the use of mercury. In about four days mercurial action was established, the disease rapidly disappeared, and there was a perfect restoration of his mental and bodily functions.

Remarks.—It may be alleged, that the treatment for paralysis was in accordance with that usually employed, which is by no means disputed. It is the antecedent condition of mental disturbance, the length of time it existed in a chronic state, without being amenable to treatment by the ordinary remedies, its mitigation under hydriodate of potash, and the previous history, which seem to me to justify the assumption, that syphilitic tumour of the dura mater was the cause of this cerebral irritation. The perfect disappearance of every symptom, under mercurial action, in a period incomparably shorter than that in which cerebral disease, originating from causes of more usual occurrence, would have disappeared, confirms me in that view. As this gentleman, soon after his recovery, emigrated to a very distant region, such incontrovertible testimony of the fidelity of the diagnosis cannot be adduced as substantiates that of the previous case. I only know, that some months after his arrival in the country of his adoption, in a letter to a friend, he expressed the warmest feelings of obligation and gratitude to me for the enjoyment of perfect restoration to health. A drier, warmer, and more equable climate is undoubtedly among the agents capable of overcoming the syphilitic diathesis.

The practical conclusions I would deduce from the foregoing cases are very obvious. First, that a single symptom of cerebral disturbance, such as some form of mental disorder, may alone indicate the organic mischief in progress; a cachectic countenance may excite suspicion of its nature; but a close investigation of the previous history of the patient's diseases and symptoms is required to affix its true character. Secondly, That the employment of mercury, in this late stage of syphilis, is unattended with those formidable consequences of which we are so strongly forewarned as forbidding its use. It is true, mercury never was pressed much beyond the disappearance of symptoms, and whenever employed, it was as the last resource of extreme peril and emergency, all other means having failed. But no measure of bodily exhaustion, even such as I feel the feebleness of words to represent, has deterred me from its employment once I had discriminated the disease, guarding myself, however, by a measure and method of use appropriate to the case.

I subjoin here the following contrast and comparison of mercurial action on the constitution of patients suffering from primary and tertiary syphilis.

When mercury has been administered for a primary sore, in a constitution of unexceptionable health and vigour, it will not be disputed that its action will always be marked by physical reduction and depression. There will be loss of flesh and weight; muscular elasticity and power will diminish; the countenance becomes pale; the skin clear, and prone to perspire on slight exertion; the intellect unfitted for sustained effort. All these will occur, although the diet be adequately nutritious and abundant for a person in health, and moderate exercise and exposure to the atmosphere be still permitted. But under confinement, all these constitutional evidences of mercurial action become more aggravated, together with, in all ordinarily, some degree of mental depression, and in some, alarming physical and mental prostration. In fact mercury in all cases visibly acts as a poison in a lower or a higher degree. Under this constitutional ordeal, the animal

poison which is secreted by the sore is in some inexplicable mode neutralized, or prevented from entering into the circulation and contaminating the system. Such is a feeble and imperfect outline of the phenomena indicative of mercurial action in a sound person affected with a primary sore. But all these phenomena are directly reversed when the body has long sustained the inroads of the syphilitic poison, under which the powers of the constitution become prostrated. The more advanced the stage, the wider the range of involvement, the more signally does mercury, if *appropriately and judiciously employed at the right conjuncture*, exhibit its strange conversion into a tonic, and its prompt and most decided action as a restorative. All the functions of life are rapidly roused into a vigorous effort—refreshing repose revisits the patient, worn with pain and exhausted by sleepless nights—appetite and digestion return—blood is quickly regenerated—the muscles recover tone and power—the eyes lose their dull and desponding cast, and resume brilliancy and expression—the mind becomes hopeful and exhilarated—flesh and weight are added, yet no medicine has been administered except mercury. How to explain and reconcile these antagonising agencies I do not purpose to attempt: the facts I have repeatedly witnessed in several other forms of syphilitic cachexia, independently of the cases now related.—*Dublin Quarterly Journal, Feb.* 1852, *p.* 53.

17.—EPILEPSY AND THE EFFECTS OF LEAD POISONING.

By Dr. R. B. Todd, F.R.S., &c.

[The subject of the following case was admitted in a state of profound epileptic coma; and, from October the 18th to 20th, had five convulsive fits, accompanied and followed by coma, which continued for two days, when he began to recover:]

The long continuance of a state of profound coma is always calculated to excite anxiety in the friends and attendants of a patient, and to create fear lest some extensive mischief shall have been done to the brain. In this case there were two circumstances among others which greatly encouraged me to believe that no serious organic lesion existed in that organ. The first of these was, that the coma was accompanied with convulsive fits. Now you will not understand me to say that I do not apprehend danger from such paroxysms of epilepsy as this man has had; but that, when epileptic convulsions and coma occur together, and where there is no hemiplegic paralysis, we have a good deal of presumptive evidence that there is no apoplectic clot, or other organic lesion likely to damage the brain permanently. Thus we were led to ascribe both the coma and epilepsy, not to the pressure of a clot upon or within the brain, but probably to one and the same cause, which cause was suggested by the second circumstance to which I have referred—namely, that his urine was scanty in quantity, and highly impregnated with albumen.

Thus I viewed the case as one of those in which the cerebral affection was due to the presence of some irritating matter in the blood which ought to be eliminated by the kidneys. There are very good grounds for believing that urea in the blood is capable of affecting the brain so as to cause coma and convulsions. Other substances retained in undue quantity may produce the same effect, for aught we know; and certainly, coma and convulsions may occur in cases where we have no evidence of the presence of urea in the blood; but it is quite as certain that, when the kidneys fail in their action to secrete only an ounce or two of urine in the day, instead of thirty or forty ounces, coma and convulsions are very apt to ensue.

And in such cases we get proof of the presence of urea in the blood as we have done in this case. The following is the method which was pursued for this purpose:—A blister was applied to the back of the neck; and when it rose, the serum was carefully collected, and tested for urea. This whole quantity of serum was evaporated to dryness over a water-bath, and the residue was extracted with alcohol, which is a ready solvent of urea. The alcoholic extract is then evaporated to dryness, and a little water added so as to make a spongy mass, which is plunged *into a freezing* mixture and a few drops of pure nitric acid are added. If urea be

present, the characteristic crystals of nitrate of urea are soon formed in the solution, and are at once recognised either by the naked eye or by the microscope.

Our patient is an intemperate man, and filled the place of waiter at a low tavern. We have not been able to trace any immediate exciting cause of his present attack, except exposure to cold shortly before. It is probable that, under the influence of his habits of intemperance, renal disease had been making insidious progress for some time; but, on exposure to cold, the action of the skin being checked, an acute affection of the kidneys was induced, those organs became highly congested, their tubes filled with epithelium, allowed but a small flow of urine, already curtailed in its quantity and charged with serum, whence its albuminous impregnation. This defective excretion of urine allows urea to accumulate in the blood, which, passing to the brain, so far disturbs the nutrition of that organ as to excite convulsions and coma.

As soon as recovery from the state of coma took place, we observed a paralytic affection of the upper extremities, and of those alone; the legs were in no way involved; nor were all the muscles of the upper extremities paralysed; those of the arm and shoulder were only very slightly affected; the muscles of the forearm were chiefly engaged, and of these the extensors were most distinctly paralysed. All were wasted; but the extensors most so, as was obvious from the hollow which existed over those muscles on the back of the forearm. He could flex the wrist and grasp with his fingers, but very feebly, and he was wholly unable to extend the wrist or the fingers. When the arm was stretched out from the trunk, the hand hung as if lifeless from its articulation at the wrist. Both upper extremities were affected in precisely the same way, although not precisely to the same degree; for the right forearm was evidently weaker than the left. The muscles which form the prominence of the ball of the thumb (the *thenar* eminence of surgical anatomists) were also paralysed and greatly wasted, so that the power of flexion, or extension, or adduction of the thumb, was almost entirely destroyed.

It is plain that in this case there were two classes of symptoms with which we had to deal—a more acute class, which yielded pretty readily to treatment, and which must have destroyed the patient sooner or later if they had not given way; and a more chronic, which remained after the first had been removed, and which seemed to have no tendency to shorten life. The first were the coma and convulsions—the second, the palsy.

Viewing the first and acute class of symptoms as indicative of the state to which I have given the name *renal epileptic coma*, I adopted a treatment actively eliminatory, with the view to remove by other channels, as much as possible, the material which was irritating the brain. The channels through which I endeavoured to conduct this noxious matter were the intestinal mucous membrane and the skin.

His head was shaved, and he was freely blistered over the scalp; mustard cataplasms were applied to the back of the neck, and he was very freely purged. The best purgative to use in such cases is elaterium, because it acts promptly, and produces liquid stools, carrying off large quantities of serum from the system, which, no doubt, as that obtained from the blister did, contained urea. Warm baths, or hot air baths, may often be used with great advantage in these cases; but I did not use more than one of each in our patient Clarke's case, as I thought him too weak to subject him much to remedies which, especially the latter, have a decidedly depressing influence.

He was admitted on the 18th of October; on the 22d he was quite free from any comatose symptom, and there now remained to be dealt with the condition of the kidney (the *fons et origo* of the cerebral disturbance), and the paralytic state.

The urine had increased considerably in quantity: it was still, however, very highly albuminous, becoming almost solid by heat, and of low specific gravity; and, under the microscope, contained casts of tubes and epithelium, and some blood particles. It was plain that, whatever might have been the former state of the kidney, it was *now* very much irritated, and that the defective secretion and albuminous impregnation were due to this.

The condition of the kidneys has improved greatly in this case under the use of purgatives, at first elaterium, and afterwards compound gamboge pill, and also of warm baths; so that now the urine contains very little albumen; but the paralytic state has remained very little changed.

It exhibits precisely the character of that form of palsy which results from lead-poisoning, more commonly known as the "painter's wrist drop." All the very characteristic signs of this form of palsy were as well marked in this case as in any case I have ever seen. When you make him hold out his arms, you see both hands hang down, and he has little or no power to bring them to the state of extension.

A practical man could not see such a case without asking if the man was a house-painter. Yet we found, on inquiry, that not only was he not of that trade, but that his proper vocation, that of a waiter, did not particularly expose him to the lead contamination.

Can the palsy be due to any particular lesion of the nervous centres, independently of lead? Or is it the result of the renal disease? It cannot be the latter, for it existed before the attack of coma, and its origin dates as far back as four or five years, and was preceded by two attacks of which he gives but a very imperfect account, but which were attended with obstructed bowels and severe pain in the belly—attacks resembling lead colic. He was at this time in Devonshire; but he states that he did not drink cyder.

I do not think that the palsy can be attributed to any special lesion of the nervous centres. I know of none which would produce exactly this form of paralysis—so symmetrical—affecting particular classes of muscles in preference to others, and those of the forearm especially, and without any damage to sensation.

We are driven, then, to attribute the palsy to contamination by lead; and, in confirmation of this, we have found the blue margin to the gums, pointed out first by the late Dr. Burton. The blue line is not uninterrupted, for he has lost several teeth, and at these points it ceases; but wherever there is a tooth, or a stump projecting above the gum, there the blue line is distinct.

But the difficulty in this case has been to explain how the lead came to be introduced into the system. It is true we are exposed to this contamination from the water we use, and we shall be so as long as there are leaden cisterns and leaden pipes to convey the water. In time it may be hoped that glass may be substituted, or some other substance not likely to yield up poisonous matter to the water. The wonder is, not that an occasional case of this kind occurs, but that they are not infinitely more frequent. Some people, no doubt, exhibit the idiosyncrasy of being strongly affected by very small quantities of particular poisons, which it is generally necessary to administer in much larger doses to produce the specific effects. We see this often in the use of mercury and of iodide of potassium. It is possible that this man may have this idiosyncrasy as regards lead.

But I think we have a better explanation than this. It appears that a part of his duty has been to clean and keep bright the pewter pots belonging to the public house to which he is attached. This he does by friction with the hands. Now pewter very commonly contains lead in considerable quantity; and no doubt the frequent contact of this with the hands would lead to a gradual absorption of a sufficient quantity of the metal to produce the poisonous effects; or the repeated frictions might cause the separation of minute metallic particles, which might be inhaled.

Now the palsy from lead is most probably due primarily to the contamination of the muscles by the lead: they thus suffer in their nutrition, and the nerves, especially the motor nerves, suffer in consequence of their comparative inaction. Doubtless, after long exposure to the contaminating influence, the nervous matter itself will become poisoned, and thus in such cases brain symptoms ultimately show themselves. But the palsy may be regarded as a form in which the loss of motion is in the first instance due to a diseased state of the muscles themselves,—not as is generally the case, to disease of the nerves, or of the nervous centres.

The theory of lead palsy, which refers it to lesion of the spinal cord or brain, evidently will not account for the phenomena. It will not account for the muscles of the forearms being chiefly affected; nor will it explain the greater amount of weakness on one side than on the other; nor the greater palsy and wasting of extensors than flexors; nor the special affection of the muscles of the thumb; nor, in fine, the almost total exemption of the nerves of sensation amid so much injury *to the motor* function. Moreover, I think it may be affirmed with perfect truth that

a lesion of the spinal cord sufficient to create so much palsy of the upper extremities as we often see in such cases as these would necessarily affect other parts also; whereas this man Clark has full power of his lower limbs, his mind and senses are perfectly clear, and he has no symptom of any nervous affection besides the palsy of the upper extremities.

I have already explained to you the treatment adopted in this case for the renal disease. Fortunately, that necessary to remedy the effects of the lead poisoning is much of the same kind. He has had frequent sulphur baths, with evident advantage to his general health. I have seen many instances of great improvement to the state of lead cachexia following the long-continued use of sulphur baths. He has also gone through a long course of galvanism with very little benefit: he has had one arm fixed upon a splint, according to Dr. Pemberton's plan, whilst, for the sake of comparison, the other arm was left free. It was evident after this treatment that the arm which had been bound up was worse than before. On the whole, the lead palsy is very little better than on his admission; nor is it to be expected that it should have been otherwise in so short a space of time. The poison of lead damages the muscles so much, that it requires a very long time before any marked change takes place: and I do not know that any one remedy exercises a specific influence; but there can be no doubt that all those hygienic means which contribute to promote a sound general nutrition are the most useful in aiding the recovery of the patient.—*Med. Gazette, Dec.* 19, 1851, *p.* 1045.

18.—ON TRAUMATIC SPASMS.

By William Colles, Esq., Surgeon to Steevens' Hospital, &c.

[Mr. Colles attributes the difference of opinion as to the description, treatment, and results of tetanus to the fact that two distinct diseases have been confounded together. Mr. Colles then proceeds to describe some distinct spasmodic affections he has seen attending injuries, and as these generally attended fractures, he directs his attention chiefly to this class of cases. He says:]

There are, I think, four distinct affections of this kind, which differ in their time of invasion, their progress, and their treatment, and yet have been more or less confounded with tetanus.

The *first* is the least important and least dangerous. It comes on immediately after the receipt of the injury, before any adjustment of the parts has been attempted; on the least motion of the patient, or handling of the parts by the surgeon, the muscles of the limb affected are thrown into a sudden spasmodic action, causing a jerk in the part, and an increased displacement of the fractured ends of the bone. This is attended with severe stings of pain. This sudden momentary spasm ceases and the part remains quiet, the muscles soft and relaxed, and the patient free from pain, until it is again induced by any motion imparted to the limb either by the patient or surgeon. The occurrence of this spasm has been attributed by some to a sharp spicula of bone irritating the muscles, by others to the want of support or of points of resistance to muscular action. The subject has been fully discussed by the late Dr. Houston, in a valuable paper on Fractures, in the former series of the 'Dublin Quarterly Journal of Medicine,' Vol. VIII., p. 477. I shall therefore only observe on the treatment of it, which is very simple. We have merely to place the limb in such a position as will allow the muscles to be most relaxed, to restore the fractured ends of the bone to their natural position; and to retain them so by firm, but not too tight dressings. A full opiate will assist the local treatment, and this may even at times require to be preceded by the abstraction of a few ounces of blood.

This is never a formidable affection, and seldom continues beyond the second or third day, though up to that time the surgeon must avoid disturbing the parts as much as possible, as the tendency to spasm may still remain, and the least roughness in manipulation may cause a recurrence of spasm, and a renewed displacement of the parts.

Such is the first form of spasm I mean to describe; we should expect that it would be readily known to the surgeon; yet we have evidence that it may be, and

has been confounded with another very different form, to which I shall next direct attention.

This, the *second* form, is very different from the preceding, being, perhaps, the most severe and rapidly fatal of all these spasmodic affections. It generally commences about the third or fourth day after the receipt of the injury, at the time we would expect inflammation to be fully and fairly established, and all danger from its excess or irregular action to be passed. At first the patient, while dozing to sleep, is suddenly awakened by a severe jerk in the limb, and a very acute but transient attack of pain, which returns every time he endeavours to compose himself to rest, so that at last he will use every exertion of his mind to prevent any tendency to drowsiness, such is his horror and dread of the recurrence of the spasm. This spasm is at first confined to the parts about the injury; the muscular action during it is very violent, almost always causing a displacement in the fracture, which cannot be retained in its position by any application; in fact, any restraint seems only to aggravate the severity and rapidity of the seizures, so that we are compelled to leave off all splints and dressings; and the pain attending it is so sudden and excruciating, that even the strongest-minded patient cannot avoid giving utterance to most agonizing shrieks. When this sudden jerk of muscle and attack of pain have ceased, the parts return to their natural state; the muscles become soft and relaxed; the limb free from pain; and the patient can move, speak, and swallow almost as usual.

The spasms commence, as I have said, in the injured limb; recur at uncertain, irregular, and generally long distant intervals; but as the disease advances, they return more frequently and regularly. At first the injured limb (*e. g.* the leg) only is affected; as the disease advances the spasms attack the muscles of the thigh, extend to the abdomen, to the arm of that side, then gradually to the other side of the body, until at length every voluntary muscle is in violent action during this momentary spasm, distorting the patient's body and the injured limb in a most fearful manner.

As regards the constitutional symptoms, the pulse becomes increased considerably in quickness, but not in force; the temperature of the body is not increased at first; as the disease advances, a perspiration breaks out, and becomes more profuse, cold, and clammy towards the end, when the patient's mind, hitherto collected, begins to wander.

This form of spasm is most rapidly fatal, generally terminating in death, apparently from exhaustion, between the second and the sixth day from its invasion. And it is remarkable, that the severity and rapidity of its course is not connected with the severity of the injury: I have seen it follow a simple fracture of the forearm. During its progress it is, in my experience, quite uncontrollable by medical means.

The largest opiates have no other effect than to stupefy the patient; they do not prevent the recurrence of the spasm. Even chloroform, administered both externally and internally, has failed to effect more than a slight temporary alleviation from pain. I believe there is only one remedy that holds out any certain prospect of saving the patient's life, and that is, the removal of the limb. And even this, to be effectual, must be performed before the spasms become general or frequent.

An examination of the limb after removal will frequently explain satisfactorily the cause of this affection. We in general find that a portion of nerve has become impacted between the fractured ends of the bone, and that it has become severely pressed on and subsequently inflamed. And to this we must attribute the spasms and pain, although we cannot so readily explain how they become so general over the body, and so rapidly fatal.

[Mr. Colles cannot from his own experience speak as to the value of cutting down and dividing the nerve leading to the parts; he should infer that in some cases it may be successful, and the assistance of chloroform in the operation may be very valuable. The form of spasm above spoken of is that which Mr. Colles thinks has been most often mistaken for tetanus, and he has no doubt that success in a few cases led Baron Larry strongly to recommend amputation as a cure for tetanus.]

The third form of spasmodic affection is that denominated tetanus. This gene-

rally comes on towards the cure of the injury, about the time the sloughs have all been separated, and the wound puts on a healthy, healing aspect, that is, about two or three weeks after the receipt of the injury. It commences with stiffness about the jaws, the muscles becoming rigid, rendering the motions limited and swallowing difficult. The stiffness gradually extends to the chest, abdomen, and extremities, and this rigidity once established, does not one instant relax during the entire course of the disease; and this it is which gives to the face that peculiar expression of countenance which, once seen, can never be mistaken.

Along with this rigid state, the muscles are also attacked by spasm, in which they suddenly become more contracted. This spasm is attended with a jerk in the expiration, and a sudden pain in the scrobiculus cordis, after it the muscles return to their former rigidity, but never relax. The spasms increase in severity and in rapidity, and the disease may thus continue for two or three weeks. During its course, we find little or nothing to connect the local with the general symptoms; the wound still looks healthy, the secretions from it become very trifling; the patient does not complain of any pain there; in fact, it has happened that he has entirely forgotten the fact of his having had an open sore. On examination after death, we can find little or nothing to account for the symptoms.

As to the treatment of this spasmodic affection, I do not now purpose to offer any suggestions. I will merely remark, that if there is one point on which surgeons of the present day are agreed, it is, that amputation has no influence on the progress of the disease. Hence the great importance of distinguishing these two last forms of spasmodic affection, so as not to treat them as the same. In order to show the more important distinction between them, I will compare their more essential differences, and thus attempt to render the diagnosis of each clear and distinct.

1st.—The second form of spasm which I have described comes on in three or four days after the accident.	Tetanus seldom appears before the second or third week.
2nd.—It commences by spasm in the limb injured.	Tetanus by stiffness of the throat.
3rd.—In the intervals between the spasms in the former, the muscles are quite relaxed, and the patient can swallow and move with comparative ease.	In tetanus there is, as the name implies, constant rigidity, almost preventing swallowing or motion of any description, and giving the peculiar expression of countenance.
4th.—In it the pain is chiefly in the wound, and is most excruciating during spasm.	In tetanus there is no pain in the wound, but a pain, not very severe, at the scrobiculus cordis.
5th.—It runs its course in three or four days.	Tetanus may continue for as many weeks.
6th.—In it amputation holds out the only prospect of relief.	In tetanus amputation is perfectly useless, if not injurious.
7th.—The former seems to have a local origin.	Tetanus is more of a constitutional affection.

Such are the leading distinctions between these two forms of spasmodic affection; and although the symptoms may not every one of them be so distinctly defined as I have here denoted, yet the majority of them are always present, and quite adequate to enable us to form a correct judgment as to the line of treatment to be adopted.—*Dublin Quarterly Journal, Feb.* 1852, *p.* 33.

19.—CHLOROFORM IN INFANTILE CONVULSIONS AND OTHER SPASMODIC DISEASES.

By Professor SIMPSON, Edinburgh.

[As the majority of convulsive attacks in infants depend upon sympathetic or functional derangements, and not on structural changes—the first indication is to discover and remove sources of irritation; and the second, to reduce the super-irritability of the excito-motory system. In the more chronic cases iron and zinc

are used; in the more acute ones, antispasmodics, such as opium, hyoscyamus, and musk. Dr. Simpson gives the following case:—The Viscountess —— was confined on the 7th October. On the 17th of the same month, the child was observed by the nurse to have two or three times during the day twitchings in the muscles of the face. On the two following days these increased in frequency and extent:]

On Monday the 20th, the convulsions became far more violent in their character, were more prolonged in their duration, and were repeated with much greater frequency. They continued with little change, and no abatement in their intensity or frequency, for the next fourteen days. Sometimes they affected the right side of the body much more severely than the left. In the meantime, Dr. Scott and I tried a great variety of means for their relief; but all in vain. The bowels were well acted upon with mercurials, magnesia, &c.; and every separate function attempted to be brought as near as possible to the standard of health. A new wet-nurse was procured, lest the milk might perchance have been proving, as it sometimes does, the source of irritation. The child was placed in a larger and better ventilated room. Ice and iced water were occasionally applied to the scalp. At one time, when the fits became unusually prolonged, and were not only accompanied, but followed for a time, by much congestion in the vessels of the scalp and face, and an elevated state of the anterior fontanelle, two leeches were applied. Liniments of different kinds were used along the spine. Musk, with alkalies, was given perseveringly for several days as an antispasmodic; and small doses of opium, turpentine enemata, &c., were exhibited with the same view. All these and other means, however, proved entirely futile. As I have already stated, it was on Monday the 20th October that the fits first assumed a severe character, and they continued without any amelioration for about fourteen days from that period, recurring sometimes as frequently as ten or twelve times in an hour. At last the child, who had hitherto maintained wonderfully his strength and power of suction, began to show symptoms of debility and sinking; and during the fifteenth and sixteenth days of the attack, the fits became still more violent, and more distressing in their character. They were now accompanied with moans and screams that were very painful to listen to; symptoms of laryngismus and dyspnœa supervened towards the termination of each fit; and in the intervals the respiration, as well as the pulse, continued much quickened.

During these two last days of the disease, the exhaustion became so great, the dyspnœa in the interyals so distressing, and the fits so very violent and constant (seventeen were counted in one hour), that Dr. Scott and I gave up all hopes of the possible survival of the infant. We had exhausted all the usual means of relief. Ultimately, but much more with the view of abating the screaming, laryngismus, and other distressing symptoms under which the little patient was suffering, than with any great hope of permanent relief and cure, I placed the child, on the forenoon of the 5th of November, for about an hour under the influence of the inhalation of chloroform. During this hour there was no recurrence of the fits; but in a short time after the withdrawal of the action of the anæsthetic, the convulsions recommenced with their old violence and frequency. The benefit, however, was sufficient to encourage a longer repetition of the remedy; and from four to eight o'clock in the afternoon of the same day, my assistant, Mr. Drummond, placed and kept the child again under the influence of chloroform, a few inhalations from time to time, of a very small quantity of the drug sprinkled upon a handkerchief, and held before the face of the infant, being sufficient for this purpose. It was specially applied at any threatening of the recurrence of a fit, and during the four hours in question, all convulsions were in this way repressed. When the child was allowed to waken up at eight o'clock, it took the breast greedily, and continued well for upwards of an hour, when the convulsions again began to recur. At last, about twelve o'clock P. M., it was again placed under the inhalation of chloroform, and kept more or less perfectly under its action for upwards of twenty-four continuous hours, with the exception of being allowed to awaken eight or ten times during that period for the purpose of suction and nourishment. During most of this period it was carefully watched by Mr. Drummond, and at last the nurse was intrusted with the duty of adding the few drops of chloroform to the

handkerchief, and exhibiting them at any time the child was offering to awaken or become restless.

After this long continuation of the chloroform, the child, on being allowed to waken up, as usual drank greedily at the nipple, and immediately fell back into a quiet and apparently natural sleep. The chloroform and all other formal medication was in consequence discontinued: and *from this time there was subsequently no recurrence whatever of the convulsions.* In about ten days the infant was removed with the family to the country. I have, within the last two days (December 18), seen the child as it was passing through Edinburgh. It was strong, plump, and well grown for a child of ten weeks, and was, in fact, revelling in the best of health.

In exhibiting the choloroform to this infant, ten ounces of the drug were expended: but of course a very large proportion of this quantity was lost by evaporation, in consequence of the mode in which it was employed.

I have known the inhalation of chloroform similarly useful in other cases in arresting infantile convulsions; but I am not acquainted with any instance in which the patient was so young as in the above instance. In the adult also, especially in cases of puerperal convulsions, I have now repeatedly seen the inhalation of chloroform as signal and satisfactory in its antispasmodic power over the convulsive fits, as it was in the little patient whose case I have described. Tetanus and epilepsy have been temporarily arrested and controlled by it. And perhaps it will yet be found one of our most certain and beneficial therapeutic means in the functional forms of those different convulsive or spasmodic diseases that are produced either by an undue excitability of the true spinal system, or by distant morbid irritations acting through this—the excito-motory system. Such reflex convulsive or spasmodic affections are, as is well known, particularly common in infancy and childhood. I have seen its use arrest laryngismus, colic, hiccup, &c.; and cases have been detailed to me of its occasional successful use in asthma, spasmodic urethral stricture, &c. But there is one common and too fatal spasmodic disease, almost confined to the period of childhood, in which I have seen anæsthetic inhalations successful in arresting and controlling the paroxysms, and where probably a more extended and persevering use in the employment of them would be found to be attended with beneficial effects. I allude to hooping-cough. I have known chloroform inhalations greatly abate the irritability of the cough attendant upon phthisis, &c. But with others, I have scrupled to use chloroform inhalations in hooping-cough, under the fear that they might possibly increase the great predisposition which exists in this affection to pneumonic inflammation, or aggravate that inflammation if it were already present. This *a priori* reason, however, against the use of chloroform inhalations as an antispasmodic in hooping-cough has been of late set aside by the observations and experience of different German physicians. In a paper, containing some remarks relative to the medical uses of chloroform, published in the 'Monthly Journal' for December, 1847, in addition to its employment as an antispasmodic, anodyne, &c., I suggested the possibility of the drug acting as a contra-stimulant in some inflammatory diseases, and particularly in those of a painful kind. Latterly we have had records published of its employment in upwards of 200 cases of pneumonia in German practice. Out of 193 cases of pneumonia treated with chloroform inhalations by Wachern, Baumgärtner, Helbing, and Schmidt, 9 patients died, or the mortality amounted to 4¼ per cent. Dr. Varrentrapp has given chloroform in 23 cases of pneumonia in the Frankfort Hospital. One of these 23 patients died. The detailed results in the other 22 cases seem to have been sufficiently satisfactory. At all events, the effects of the chloroform inhalations upon the cough, expectoration, &c., and upon the general course of the disease, would appear to show that we need have no fears of deleterious effects from it as far as regarded the chance or existence of pulmonary inflammation; whatever advantages we may derive from it in relation to its prevention of that inflammatory state by allaying the cough, keeping the lungs in a relative state of quietude, and abating or restraining the successions of characteristic spasmodic attacks. I speak of course of the more severe cases of pertussis; for the milder forms of it require care merely rather than actual treatment.—*Monthly Journal of Med. Science, Jan.* 1852, *p.* 40.

20.—*Tic Douloureux remedied by Operation.* By Dr. ALLAN.—A young woman, aged 25, was brought to Dr. Allan, a perfect martyr from tic, beginning over

the right eyebrow, and extending over the face. Her complaint had been of six years' duration, and was gradually becoming more severe. It commenced with characteristic exactness at a certain hour in the morning, at times changing its time of visit until night. On feeling the pained eyebrow, the cellular substance on both sides seemed very thick. A hard body was detected; and on cutting down, a calcareous concretion was dislodged from its position immediately over the supra-orbital foramen, where it was attached to the nerve. Since its extraction, the girl has been comparatively free from pain. Dr. Allan asks:—"*May not inveterate tic be often caused by similar deposits in inaccessible portions of nervous channels?*"

The concretion removed was the size of a large pea, and was covered with a pellicle of cellular tissue; it was hard, gritty, unorganized, and consisted entirely of carbonate of lime.—*London Journal of Med. April* 1852, *p.* 362.

21.—*Treatment of Sciatica.* By C. R. BREE, Esq.—Mr. Bree suggests a trial of the following treatment: he is desirous of obtaining statistical information on the subject. Sciatica, whether connected with rheumatism or not, and not dependent upon mechanical causes, as accumulation of fæces, tumours, &c., may be cured in fourteen days, by rubbing along the affected nerve, from its origin downwards, half a drachm of veratria ointment (gr. v. to oz. ss.) every night at bed-time. The friction to be performed with a horse-hair glove until severe tingling is induced.—*London Journal of Med., April* 1852, *p.* 379.

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22. *Cases of Sciatica and Neuralgia successfully treated by Acupuncture.* By Dr. H. S. BELCOMBE, Senior Physician to the York County Hospital.—[Dr. Belcombe, of York, gives us the following cases illustrative of the treatment of neuralgia by acupuncture:—]

Case I.—A lady of fortune, aged 65, stout, full habit, partial to luxurious living, but a water drinker; indolent as to exercise, except in a carriage, was attacked early in the spring with severe pains across the loins, shooting down the thighs, causing great inconvenience night and day. The bowels were confined; urine moderate in quantity, turbid. The means used gave very partial relief; presently the pain centred just between the tuber ischii and trochanter major, darting down to the knee and ankle; spasmodic action of the calf of the leg, sometimes very great. A needle was inserted just at the seat of pain to some depth, and another two inches lower in the same direction. Both were kept in for two hours and then withdrawn; the pain entirely subsided, and has not hitherto returned.

Case II.—A retired tradesman, aged 70; very corpulent; moderately active; fond of the pleasures of the table, but sober. Symptoms much the same, only the pains resembled more the neuralgic tic, and rest was impossible. The needles were introduced three different times before success was obtained; but when obtained it was complete.

Case III.—A butcher, middle-aged, tall, stout, plethoric; very active in his pursuits, very moderate in his diet. The pains first commenced in the loins, then the shooting down the thighs, preventing his going to market or attending in his shop; then the concentration in the sciatic nerve. The needles completely relieved the pain after one insertion.

Case IV.—A married lady, in easy circumstances, aged 45; no family; very active in her mode of life, very temperate in her living, yet inclined to corpulency; for two years she had been seldom free from this vexatious sciatica, rendering her existence very uncomfortable. Many measures had been tried in vain for relief, and it was only by the effect of persuasion she was induced to consult me. The insertion of the needles was attended with complete success; the pains have not hitherto returned, and her movements are as free and easy as in youth.

I might add three more cases where the disease was situated in the nerve of the upper extremity, though the effects were most sensibly felt in the wrist, the pain *in the shoulder being only* casually mentioned or thought of. The insertion of the

needles has been always successful. In these, belladonna plasters were also applied to the wrists, though more I think to gratify the patients, who were resolute the seat of disease was there, than from any expectancy I had of relief, though in many cases I have full confidence in the remedy. One case I may mention. A countryman had a paralytic seizure two years back, which deprived him of the power of speech, though the limbs perfectly recovered. I was sent for lately early in the morning. I found him incapable of using his right arm, which seemed to hang motionless, and his friends were persuaded he had had a paralytic attack in the night. From learning somewhat of the history, and examining attentively the limb, observing there was a more clonic spasm than paralytic degeneration, I was certain it was rheumatism, and had a belladonna plaster placed upon the shoulder and muscles of the neck. Other medicines, I think, were calomel and opium. The intelligent countenance, the clear eye, following me attentively as I made my examination, strengthened my judgment as to the nature of the attack. He recovered very shortly.

The medicines I gave in all the cases of sciatic lumbago were acetate of potash in compound infusion of senna, the patients finding much relief from the movement of the lower bowels, and pressure being taken off the kidneys. In the neuralgia of the upper extremity carbonate of iron in full dose was given.

The different medicines were given with a view of testing their peculiar operation. I am of opinion that full purgation is very useful in all cases of rheumatism, especially in the acute; and that it is more useful where the lower extremities are attacked than when the distress falls upon the upper. The carbonate of iron is certainly an excellent remedy where the chronic form prevails.—*Med. Times and Gazette, Jan.* 1852, *p.* 85.

23.—*On the Treatment of Chorea.* By Dr. SÉE.—Dr. Sée enters into a critical examination of the various modes of treatment, and makes some very interesting observations upon the employment of gymnastic exercises and sulphureous baths. *Gymnastic exercises*, suggested long since by Darwin and Good, have been recently employed at the Hôpital des Enfans with the most marked success; and as the subject is of great interest just now, when we are commencing the establishment of children's hospitals in this country, we may state the general results of their introduction into that of Paris to a much later date than M. Sée's essay refers to. They were first employed there in 1847, under the guidance of M. Laisné, gymnastic professor at the Polytechnic School, their effects being first tried on scrofulous children. Commencing with simple movement of the legs and arms, accompanied by appropriate songs, the children's progress was so rapid, that they were soon able to employ the orthopœdic ladder, the parallel bars, and other machinery, in succession. By the twentieth lesson they were exercised in wrestling, and afterwards in running, special exercises being devised for the lame. From the first lesson the children became fired with emulation, and movements which seemed impossible were soon executed with ease and pleasure. A marked amelioration was speedily observed, their countenances becoming animated, their flesh firm, their voices stronger, their appetites keener and more regular; glandular swellings, which had long resisted all treatment, were resolved, and fistulous sores that had been open for years closed up. The lessons, one hour each, were given three times a week; and in the intervals the children amused themselves by repeating such of them as did not require machinery. The entire appearance of the wards was changed. In place of the children sitting or lying about listlessly, they were now seen practising their marches to their songs, running, wrestling, and trying to surpass each other,—the girls nowise yielding to the boys. The beneficial agency of such activity imparted to these naturally indolent and apathetic subjects may easily be conceived. These favourable results led to an enlargement of the sphere of the experiment; and the treatment was extended to nervous affections, partial paralysis, rickets, and especially *chorea*. Since 1847, there have been ninety-five children suffering from chorea, sometimes so obstinate as to have resisted the most varied treatment, cured either by this means alone, or by its conjunction with other means; and during the four years, no accident whatever has resulted from the employment of the exercises. Dr. Sée states, that in applying them to chorea, care is taken to graduate them according to the severity of the case; and that they are repeated daily, but not for more than from fifteen to twenty-five minutes, so as

not to induce fatigue and palpitation. Improvement is sometimes seen after the first lesson, and at latest after the fifth or sixth; so that at the end of a week, we can judge whether the means is likely to prove efficacious; and if manifest improvement has not then taken place, it is doubtful whether the cure will be thus effected, or if it is, it will be so only after a long time. The worst as well as the slightest cases have reaped equal benefit, the cure in the favourable ones only requiring a mean of twenty-nine days, and old or relapsed chorea being more amenable than recent. Dr. Sée has found that when other remedies are conjoined with the gymnastics, the proportion of cures is less, and the period of their attainment later; and he recommends no other adjunct to be employed than good diet.

Sulphureous Baths, as devised by M. Baudelocque, is another valuable means, fifty-eight rapid and definitive cures having been obtained in sixty-five cases. Thirty drachms of sulphuret of potash are added to each bath, which is employed for at least one hour daily, at a temperature of 91°. Generally amelioration occurs after the second or third bath, but sometimes not until after twelve or fifteen days, a mean of twenty-two days having served for the cure of fifty out of fifty-seven cases. Where the cure is retarded, it ordinarily depends upon the patient's powers being lowered by other remedies or insufficient diet, upon irritation of the skin induced by the bath, or upon acute irritation of the internal serous membranes—circumstances contra-indicating the baths while they continue. The conjunction of other remedies retards rather than aids the cure. Deducting the cases in which the bath was improperly used under the above circumstances, there remain but nine true failures in eighty-one cases, these being almost all recent or rheumatic choreas. —*Brit. and For. Medico-Chirurgical Review, Jan.* 1852, *p.* 75.

24.—ON LARYNGISMUS AND ITS DIFFERENT KINDS;—ON ITS TREATMENT BY TRACHEOTOMY; AND ON THE MODE OF PERFORMING THIS OPERATION.

By Dr. Marshall Hall, F.R.S., &c.

The object of the present paper is to treat of laryngismus and its different kinds; of its treatment by tracheotomy; and of the best mode of performing this operation.

The term laryngismus has hitherto been confined to that form of malady which has been designated laryngismus stridulus, as seen in infants. Even in this case, laryngismus is not a *disease*, but a *symptom* of a more general affection; and as a symptom, it is not limited to any one form, to any one disease, or to infancy; on the contrary, it occurs in various forms, in various diseases, and in adult age as well as in the earliest period of life.

In whatever cases laryngeal phenomena are observed, the affection, when a mere *symptom*, must be termed *laryngismus*, and its forms and kinds should be distinguished and investigated. The disease, when the same phenomena arise from inflammation seated in the larynx, I need scarcely observe, is designated *laryngitis*.

Laryngismus, thus defined, is observed under various circumstances:—

1. In apoplexy, after convulsion, in the effects of narcotic poisons, in the effects of intoxication, in deep sleep, it occurs in the form of *laryngeal stertor*, and may be distinguished as *apoplectic*.

2. In epileptic, in puerperal convulsion, in the convulsive affections of infancy, as an effect of the poison of strychnia, in choking, &c.; it occurs in various forms of more or less sudden closure of the larynx, and may be distinguished as *spasmodic*.

3. Some cases of the laryngismus stridulus were referred by the late Dr. Hugh Ley to the pressure of enlarged glands in the neck on the inferior or recurrent laryngeal nerve. If such a case does really exist, and it may, though not so frequently as was supposed by that author, it may be properly distinguished as the *paralytic* form of laryngismus.

It is impossible to look upon a case of laryngismus without perceiving that, if it be severe and protracted, it must have a fatal issue. It has therefore occurred to several eminent persons, not only to suggest, but actually to perform the operation of tracheotomy, to avert the present danger.

These were cases of apoplectic laryngismus; but a spasmodic laryngismus

obviously constitutes a fearful part of the paroxysms of hydrophobia and of tetanus, and tracheotomy was first proposed as a remedy in the former of these direst of human maladies, by the late Dr. Physick, of Philadelphia; and in the latter, I think, by Mr. Herbert Mayo. These propositions were one and all suggested by the phenomena *observed;* and it cannot admit of a doubt, that, in such and similar cases of *obviously* impending danger, tracheotomy should be instituted—to *save life.*

But I have another proposition to make, of a very different character, the result not merely of observation, but of *theory* or *physiology.* It is this:—

Intermediately between the exciting *cause* of convulsion, and convulsion itself, spasmodic laryngismus, with more or less complete closure of the larynx, and violent expiratory efforts, intervene, as an essential link in the chain of such cause and its direct effects, immediate and remote. Now in some cases the effect of such convulsion is a fatal apoplexy; in others, paralysis; in others, mania; in others, especially if the fit be repeated, loss of the faculties.

My proposition is—to institute tracheotomy as a preventive measure.

This question of the application of tracheotomy in the preventive treatment of epileptic convulsion, is one involving high principles in physiology.

[Dr. Hall next makes the following remarks on the best mode of performing the operation. It is his earnest object to render it as safe and as easy as possible, so that the country practitioner of least experience need not be deterred from undertaking it.]

I first propose to make a free incision through the integument; then to separate the other tissues by means of a blunt-pointed stilet, or forceps, mechanically, without division, and then to denude the trachea. I have, then, to suggest the use of an instrument in the form of a trochar, supplied with a curved hook, which may be drawn upwards within it, and a circular cutting edge. As this last is made to revolve, the hook is drawn within it, and a circular piece of the trachea is removed.

Into the orifice thus made in the trachea, I propose to introduce a little instrument made of silver wire, much lighter than any tube, and admitting of being diminished in size for introduction and removal, and reintroduction, and so contrived as to be secure in the tracheal orifice when introduced, and allowed to expand.

I further propose that a piece of wire gauze, covered by cotton net, be fixed over the orifice, to prevent the ingress of perfectly cold dry air, especially when the easterly wind prevails.

It has occurred to me, too, that, in cases in which a protracted tracheal opening is required, the edges may be healed by means of the application of sulphate of copper, the edges of the integument being removed by the lancet or the scalpel, and brought together, when it is thought well to obliterate the orifice.

I may now observe, in conclusion, that I have on several occasions stated, that, if tracheotomy were performed, and a tube worn in the trachea, the epileptic, the puerperal, or the infantile convulsion would be prevented, with its dire effects.

Experience is now added to theory, to confirm me in this *hope*—for it is but a just degree of caution still to regard the question as one of hope.

I now generalize the idea, and apply the reasoning to paroxysmal apoplexy, as well as to convulsion, and I hope and trust that tracheotomy will be found a preventive or a remedy of the apoplexia gravior, as well as of the epilepsia gravior.

It is singular to observe how laryngismus becomes, in both apoplexy and epilepsy, the intermediate link between the milder and severer disease, the effect of the former and the cause of the latter. The due appreciation of these facts will lead to an important principle of treatment in one of those affections, and of prevention in the other. The latter of these must be viewed in the light of a remarkable prediction fulfilled by the event.

The conclusions to which I have arrived, are,

1. That in cases of apoplexy without organic disease, the patient ought not to be permitted to die without the institution of tracheotomy.

2. That in cases of epilepsy, laryngismus with convulsion, and danger to life, mind, or limb, ought not to be permitted to continue without giving the patient the hope involved in the same operation.—*Lancet,* Dec. 6, 1851, p. 526.

25.—*Physiology of the Nervous System.*—Some years ago, M. Matteucci discovered the curious fact, that when a muscle contracts on which a nerve distributed to another muscle rests, then this latter muscle contracts likewise. M. Matteucci gave the name "induced" to this species of contraction, which M. du Bois Raymond explained, and called secondary.

M. Brown-Sequard, in following up this interesting subject, has shown:—

1. That this secondary contraction is extremely feeble whenever the inductor-muscle contracts without meeting with resistance, as after the section of its tendon, for example.

2. That a tendency to, or effort at contracting in a muscle, is sufficient to produce the secondary effect; and that the latter is directly proportionate to the energy of the former.

3. That when the irritability of a muscle is very great, mere traction of its fibres in a longitudinal direction is sufficient to induce secondary contraction.

4. That the sensitive nerves connected with a muscle in contraction are excited in the same manner as motor nerves, and according to the same ratio.

M. Brown-Sequard conceives that these facts serve to explain many obscure points in physiology and pathology. Thus we know, from experience, that the severe pain which attends fissure of the anus is aggravated by elongation of the sphincter fibres, while division of that muscle either removes or greatly alleviates the pain. This fact receives a ready explanation from the above propositions. When the sphincter contracts, the sensitive nerves running along its surface, or distributed in its interior, are excited, and, as the contractions are exceedingly violent, so is the pain very severe. After division of the muscle, however, contraction of the muscular fibres takes place without any resistance, and the pain consequently disappears. In certain cases of painful contraction, the same effects are produced. Thus, we increase the pain by endeavouring to extend the contracted muscle; we diminish it by extending the antagonist muscles; we cure it by division of the tendon.

The intense pain during labour is easily explained by the excitation which violent contraction of the muscular fibres occasions in the sensitive nerves of the uterus. The same mode of explanation applies to cramps, convulsive spasms, and neuralgic affections, in which the slightest muscular action gives rise to pain.

Finally, the "recurrent sensibility" of Majendie is easily understood, when we reflect that the contraction of a muscle excites the sensibility of nerves in contact with, or distributed through them. The anterior roots of the spinal nerves are motor, yet when we excite them we produce pain. But this arises because they produce, when excited, contraction of the muscles; and this contraction, again, produces increased sensibility or pain in the sensitive nerves connected with the muscles so contracting.—*Transactions of Biological Society.—Med. Times and Gazette, Jan.* 10, 1852, *p.* 45.

26.—CASE OF INJURY TO THE NERVOUS CENTRES.

By Dr. E. A. Parkes.

[From the great variety of injuries to which these parts are subjected, the diversity of their modes of development, and the variety of the situations of their occurrence, the difficulty of their diagnosis is acknowledged. These are sufficient reasons for presenting the following case. The patient was a soldier:—]

He is said to have been of temperate habits, and to have enjoyed good health up to June, 1849, when he had an apoplectic attack. He remained unconscious, according to his wife's account, for two or three days, and recovered from this condition with hemiplegia of the left side. At the time of his present attack, he had regained very considerable, but not perfect power over both arm and leg.

On the day previous to his admission, about two p. m., without premonitory symptoms, and being at the time in very tolerable health, he was suddenly seized with involuntary movements of the foot. Subsequently these movements increased, involved the whole leg, and began to occur in the right arm also. They continued *to increase in* violence, and about 8 p. m. had reached the degree in which we

saw them. There had been no loss of consciousness, no paralysis, no headache, no vomiting.

Twenty-six hours after the commencement of the attack he was admitted into the hospital. When I first saw the case (Sunday, Nov. 2, at a quarter to 7 p. m.) the man was lying on the bed, violently tossing about; the violence of the movements being such that I can scarcely hope to convey to you an adequate idea of them. I can only compare them with those of a violent paroxysm of mania. That it was a paroxysm of mania was, indeed, the impression produced on the mind by the first aspect of the case, and to a spectator not previously informed of the circumstance, it would have been a matter of surprise to find, on speaking to the man, that consciousness and intellect were perfect. These, in fact, were retained unimpaired from first to last.

The movements referred to, being particularly examined, were found to be the following:—First, violent movements of the right arm, of a peculiar character,—not common convulsive movements, not tetanic, but more like those of chorea. The arm was suddenly and violently thrown about in different directions, above the head, across the breast, to the side. After some minutes of this violent jactitation, a temporary cessation would occur, but only for a few seconds. By a strong effort of the will these movements could be momentarily controlled. The right leg was moving, but more feebly than the arm; the movements consisted of rapid extension and flexion, and of occasional rotation. The left arm was perfectly tranquil. The left leg was moving slightly, but this appeared to be no more than was induced by the movements of the body. Then, in addition to these movements of the right arm and leg, there were extreme movements of the trunk generally, so violent that it was impossible to tell whether they were confined to one side or not. So with regard to the neck; it appeared as if all the muscles of the neck were in action. The head was sometimes rotated, sometimes thrown violently backwards against the pillow. The effect of all these movements combined was to cause the man to roll over upon the right side, until sometimes he turned completely over. He then rolled back again, apparently by an effort of the will, but never turned upon the left side.

In addition to these movements, respiration was especially affected. It was much quickened; inspiration was short and difficult; expiration extremely prolonged, and apparently imperfect. The intercostal spaces at each inspiration became deeply concave, and during expiration convex. The delay in the pulmonary circulation was shown by extraordinary distension of the external jugular veins.

The other muscles of the body seemed little affected. The tongue was protruded well, and without deviation. There was no drawing of the face. There was no strabismus, neither were the pupils affected. He could swallow tolerably well, though with a little delay. He passed his urine twice in the hospital, and a third time made an ineffectual attempt. Articulation was distinct. The voice was a little husky, but the muscles of the larynx were perfectly unaffected.

Now, these choreic movements were the *only* symptoms. Intellect and consciousness were perfect. There was no paralysis, except such as had remained after the old attack. He could grasp firmly with the right hand, and could move the right leg at will during the intervals. He could also grasp (less firmly) with the left hand, and could move the left leg. There was no contraction of any of the muscles. General sensibility also seemed to be perfect. Then there were no special symptoms, as headache, vertigo, or tinnitus. There was no vomiting, nor had there been any. The pulse was now 120.

The questions that presented themselves here, related, first, to the diagnosis; secondly, to the treatment. With regard to the former, in most cases of cerebral or spinal affection, we have a number of symptoms, from the occurrence, absence, or relative position of which we are sometimes enabled to form a tolerably acute diagnosis. But in this case we had nothing to guide us except these choreic symptoms. Now, the first question was this: Had we here a case of chorea, implicating only one side of the body? Chorea, though it chiefly affects young persons, will occur at any age. Dr. Graves records the case of a man, aged 70, who was attacked with this disease; and Romberg, in his admirable work, gives an account of a woman 47 years of age, who was affected with chorea of the

right side. This case certainly resembles, to some extent, the present. In opposition, however, to this conclusion, we had, first, the age of the patient, which was unfavourable to the notion of chorea, though it did not exclude it; next, the fact that he had had previous disease of the brain, as evidenced by the hemiplegia; then, though we called the movements choreic, they had not precisely the character of choreic movements, and, moreover, such extreme violence is unusual in chorea.

Putting aside, then, simple chorea as improbable, in what other direction were we to look for the cause of these movements? Was there any peripheral cause capable of exciting them? A hasty examination disclosed nothing of this kind. Then, supposing that there was an organic disease of the nervous centres, what was its probable seat? We are justified in excluding the meninges, because, when these are affected, the mind especially suffers in various ways. It was improbable, also, that the lesion was situated in the hemispherical ganglia, in the optic thalami, or in the corpora striata, because the symptoms of disease occurring in these situations are pretty accurately known, and no case is on record to my knowledge, in which either softening or hemorrhage of these parts gave rise to symptoms of this kind unattended by other signs. The symptoms seemed to point rather to the lower divisions of the nervous centres. From the fact that the muscles of the face seemed quite unaffected, it appeared probable that the disease was seated below the origin of the nerves supplying these parts. Admitting this to be the seat, what was likely to be the nature of the disease? This question was even more difficult to answer than the preceding one. Of the many diseases which may occur in the brain, there were two lesions which were most likely, viz., acute softening and hemorrhage. Between the two it was not possible to decide, for in either case the same symptoms exactly might have been caused. To decide between them, we should have required the aid of other symptoms, and sometimes even when we have other symptoms, as paralysis, coma, contraction, &c., such decision is impossible. Of the two, the course of the disease seemed perhaps to point rather to hemorrhage than to softening. The diagnosis, then, appeared at the moment in the highest degree obscure; but I felt inclined to think it was hemorrhage into one side of the medulla.

But a still more important question related to the treatment. In all such cases we have to commence some treatment immediately. The diagnosis may be obscure, but, whether it be or not, something must be done at once for the relief of the patient; and in many cases of head disease, we have simply to treat symptoms. In this case we had no general rule at all to guide us. The case was unusual, and I confess that I knew nothing of it from previous experience. The man himself said, that if the movements continued he could not live; it was indeed evidently necessary to do something to control them; and, as there appeared no argument against its use in such cases, we determined to administer chloroform. The effect was very remarkable. Inhalation had not been continued above two minutes, when he fell asleep, the movements entirely ceased, the respiration became easy, and the external jugulars, previously so distended, at once emptied themselves. Inspiration became more perfect, and expiration shorter. All over the front of the chest there were dry rhonchi, chiefly sonorous and droning, the presence of which we could not ascertain before, on account of the movements. The pulse fell considerably, being now 108. The heart's impulse was strong; there were no murmurs. He remained asleep for about ten minutes, and then awoke suddenly. The *same moment* the movements recommenced with some violence, speedily increased in severity, and very shortly they were as intense as before. At the same time the difficulty in respiration returned, the obstruction to the pulmonary circulation, as evidenced by the condition of the jugulars, was again manifest, and the pulse increased in frequency. Accordingly, the chloroform was repeated, and while he was under its influence, he was bled from the arm to 10 oz. During the bleeding the pulse fell to 92. Shortly afterwards, he awoke, and the movements on the instant recommenced. After waiting for about fifteen minutes, and finding no permanent improvement from these means, we gave him twenty-five minims of tincture of opium in a small quantity of water. He swallowed the draught without much difficulty. Ten minutes afterwards (at eight p. m.) the *respiration appeared* extremely difficult, and at each expiration it was noticed that

the lips were blown out, more, however, on the left side than on the right. This blowing out, if it meant anything at all, meant that the lips on the left side were more paralytic than on the other, but at a subsequent period this difference between the two sides had disappeared. The tongue was protruded well, and there was no deviation. The pulse was 120. He now inhaled chloroform for the third time, with the same effects as before, entire cessation of movements, the pulse falling to 88, respiration becoming easy. The pupils, which had previously been moderately expanded and moveable, were now very much contracted, and moved but little. The effects of the chloroform on this occasion lasted a quarter of an hour; he then awoke, the movements immediately recommenced, and the pupils suddenly became larger. At half-past eight, the movements being still extremely violent, chloroform was administered for the fourth time. It was noticed, that while under the influence of chloroform, the blowing out of the lips was equal on the two sides. The respirations were 20, the pulse 76. Having slept for about forty minutes, he awoke, and the movements at once recurred with undiminished violence.

At a quarter to ten p.m. the dose of tinct. opii was repeated, and immediately afterwards chloroform was given for the fifth time. The same effects ensued as on each of the other occasions, the respiration became easy (22 per minute), the pulse sank to 88, and the movements entirely ceased. He now slept quietly for an hour, and on awaking at a quarter to 11, the movements immediately recommenced. Half an hour later the movements continued violent; he passed some urine. Chloroform was now administered for the sixth time. Its effect, as before, was to induce a quiet sleep which continued for an hour. At the expiration of that time, however, the movements recurred with their former severity, and continued till half-past 1 a.m., when he fell asleep spontaneously. This spontaneous sleep, *during which the cessation of the movements was as complete as when he was under the influence of chloroform*, lasted about an hour. He then awoke, and the movements recommenced. At half-past 3 he again slept spontaneously and quietly for half an hour, this sleep also being succeeded by a recurrence of the movements. From this time he continued awake, and in almost constant motion, till half-past 6, the movements, however, becoming gradually less violent. At half-past 6 he again slept for a few minutes, and as he lay asleep he appeared to be dying. The pulse was now 80—92, irregular, and more feeble than it had been. Respirations 18. When he awoke, the movements recommenced with the same characters as before, except that their violence had somewhat diminished. They were still confined apparently to the right side, or rather to the right extremities, the trunk, and neck; and the arm, as at first, was more affected than the leg. The man was still perfectly conscious. There was still no paralysis, no contraction, in fact no symptom, except these movements, which, as the strength ebbed away, became less violent. He could grasp with both hands. The tongue was protruded without deviation. At a quarter to 8 he again fell asleep spontaneously, and slept quietly for three quarters of an hour. His respiration had now become very slow, numbering only thirteen per minute, and it was observed that expiration was three or four times the length of inspiration. On his awaking the movements recurred (though now with considerably less violence), and continued within a few minutes of his death, which took place at 9 a.m., on Monday, Nov. 3rd, consciousness having been retained to the last. Cadaveric rigidity came on within half an hour after death, and was extremely well marked in all the limbs. The muscles of the calf were observed to be distinctly harder on the right than on the left side.

Briefly to recapitulate; we had here a case of choreic movements of the right arm, in a less degree of the right leg; movements of the muscles of the neck and of the trunk,—suspended by chloroform-stupor, and by natural sleep; unattended with delirium, coma, paralysis, contractions, or other symptoms connected with the head; and followed by death, caused apparently partly by exhaustion and partly by asphyxia, forty-three hours after the first twitching of the foot, and seventeen hours after admission into the hospital.

At the *post-mortem* examination, the following lesions were discovered. First, beneath the arachnoid, over the anterior part of the right hemisphere, a small quantity of blood had been effused, and was lying in three or four sulci, having penetrated deeply between the convolutions, but it had not apparently affected the

cerebral substance. Secondly, There was softening, apparently old, of the upper and posterior part of the right corpus striatum and adjoining part of the optic thalamus. Before any section was made, the seat of this softening was detected by a depression upon the surface of the corpus striatum and a kind of transparency of the ventricular membrane at that part, with, at one point, slight thickening of the membrane. The softening appeared to be old, it was of greyish-yellow colour, not red, moderate in degree; not at all like the acute red softening. Then, Thirdly, there was a slight amount of similar old softening of the left corpus striatum. Fourthly, There was hemorrhage into the left crus cerebri, about half an inch in front of the pons varolii, and nearly in the centre of the crus. The quantity of blood effused was estimated at half a drachm. It was of quite recent effusion, was clotted, and had hollowed out for itself a space in the centre of the crus. The nervous substance around it was torn, but was not obviously softened.

There can be little doubt but that the disease which had caused the movements was the effusion of blood into the left crus cerebri; but it is very remarkable that the muscles of the face and tongue, and those of articulation, should have been so completely unaffected.

Then, in addition to these lesions, there was another, which was perhaps fundamental to the whole of them. All the arteries at the base of the brain were atheromatous, in a greater or less degree; and this condition extended into some of the very smallest calibre, being, however, most marked in the larger vessels. This atheroma was disposed in considerable amount, was distinctly covered by the lining membrane, and occupied the whole thickness of the middle coat. The outer coat could be stripped from it. It occurred sometimes in points, but more frequently in plates of various sizes, many of which involved the whole circumference of the vessel. It had a yellow colour, and, under the microscope, presented no very definite elements except cholesterine and fat. There were also various round or irregular bodies, rather larger than blood-discs, not distinctly cellular, slightly yellow, slightly opaque, the nature of which was doubtful. There was very little, if any, calcification of this atheroma, at least there was no effervescence with acetic acid. At the points where the atheroma was most marked, the calibre of the vessel was large; and where it involved the entire circumference, the size became very considerable. In the intervals, the artery was generally of its proper size.

Besides this disease of the arteries, there was another and a very interesting one. Several of the arteries in and about the circle of Willis were almost blocked up with firm yellow coagula, very uniform in appearance, and without the least tinge of redness. Some adhered firmly to the artery, and could not be detached without great difficulty; others were easily detached, and then formed long, round, firm yellow masses, which preserved their shape completely when removed from the vessel. That these clots were old, was evident from their colour, from their consistence, from the fact that they never filled completely the vessel, but left a narrow channel for the blood either in the centre or at the side, and particularly from the microscopic examination. On cutting through their centre, and examining scrapings from the most internal portion, there were, besides fat and other ill-defined elements, plates of cholesterine in considerable amount. Except these plates, I could see no crystals of any kind. The origin of these clots was, perhaps, the following:—Atheroma had occurred in these arteries, and the vessels, losing their elasticity, and dilating somewhat, had favoured the coagulation of the blood. This, at least, appeared to be one cause, for the clots were especially disposed where the atheroma was most marked. Then, in addition, perhaps the blood itself was at fault, and had a tendency to coagulation. That these lesions of the vessels were fundamental to the hemorrhage,—that is to say, that an atheromatous vessel had ruptured, will be admitted; and it is probable, also, that they were connected with the previous softening. It is to be regretted, that we had not an opportunity of examining the arteries of the body generally. There can be little doubt that we should have found them in a condition resembling that of the arteries of the brain; but, unfortunately, the friends insisted on removing the body, and we were able to examine only the brain and heart.

In connexion with this condition of the vessels, it is very interesting to notice the *state* of the heart. The weight of the organ was 18½ oz.; and this excess was due *almost exclusively to an enormous* hypertrophy of the left ventricle, the cavity of

which was slightly dilated. The thickness of the walls measured a full inch at the base, and about three quarters of an inch at the apex. This hypertrophy was not caused by any obstruction at the orifice, for the aortic as well as the other valves were perfectly healthy, indeed much more so than is usually seen in such cases. It is to be presumed that the hypertrophy was due to obstruction in the general circulation, and this may have been caused by a condition of arteries generally, like that we found in the brain.

Such was this singular case, the exact counterpart of which is perhaps not to be found in the annals of medicine. On looking back upon our treatment of the case, with the result of the *post-mortem* examination before us, we may ask, Did we do all that was advisable to be done? Probably we did. In the absence of an accurate diagnosis we should hardly have been justified in more active treatment, and the chloroform, while productive of great immediate mitigation of suffering, had evidently no influence at all upon the necessary and inevitable termination of the disease.—*Med. Times and Gazette, Jan.* 17, 1852, *p.* 53.

27.—*The Vital Spot of the Medulla Oblongata.*—At the meeting of Oct. 27, of the Academy of Sciences of Paris, M. Flourens took occasion to speak of the actual locality of the vital spot, or primum mobile of the respiratory act. By several experiments he has now determined that that spot is situated exactly at the point of the calamus scriptorius, between the ventricle of Aurentius, and the junction of the V-shaped grey matter of the pyramids. This spot is, according to M. Flourens, about the size of a *pin's head;* above or below the same a sharp instrument may be thrust in without causing the respiratory movements to cease; but when the exact spot is transfixed, life ceases immediately. M. Flourens had made this communication in order that the precise locality of the nodus vitæ should be well understood.—*Lancet, Nov.* 29, 1851, *p.* 509.

DISEASES OF THE ORGANS OF CIRCULATION.

28.—OBSERVATIONS ON THE SOUNDS OF THE HEART.

By Richard Brown, Esq., Cobham; and Robert Cartwright, Esq.

[Few subjects have claimed more of the attention of physiologists than the sounds of the heart. Mr. Brown says:]

Harvey and Haller describe the contraction of the auricles as preceding those of the ventricles. Laennec conceived that the contraction of the auricles followed those of the ventricles. Turner, Corrigan, Williams, Hope, and Bouillaud, have shown the inaccuracy of Laennec's opinion. Dr. Williams investigated this subject, and his inferences were confirmed by the Committees of the British Association—namely, that the contraction of the ventricles followed immediately that of the auricles.

The first sound of the heart was ascribed by Mr. Carlile to the rush of blood into the great arteries; by Mons. Rouanet and others to the closing of the auriculo-ventricular valves; by Dr. Hope, to the collision of the particles of fluid in the ventricles; and by Dr. Williams, to the muscular contraction of the heart itself.

The second sound was ascribed by Dr. Hope to the impulse of the blood from the auricles filling the ventricles; by Messrs. Carlile, Carswell, Rouanet, Bouillaud, and others, to the suction of the ventricles, causing the elevation of the sigmoid valves, and to the reaction of the arterial columns of blood against these valves.

The experiments performed by Dr. Williams, assisted by Dr. Hope and others, in order to determine these points, proved that the first sound is produced by the muscular contraction of the ventricles, and that the second sound is caused by the reaction of the arterial columns of blood, tightening the semilunar valves at the diastole of the ventricles.

The first motion of the heart following the interval of repose is the systole of the auricle, which I consider accompanies the diastole of the ventricle; and the systole of the ventricle immediately follows its diastole, commences suddenly, and considerably diminishes the volume of the organ.

On applying the ear or a stethoscope over the præcordial region, two sounds are heard following each other; the first is dull and prolonged, whilst the second is shorter and sharper. The first sound is produced during the diastole, and the second during the systole of the ventricles; and in support of this theory I will briefly state the circumstances under which this opinion was formed.

Some few weeks since, attending a patient labouring under increased action of the heart, and whilst conducting the examination, I could distinctly appreciate the inward current of blood from the auricle to the ventricle, producing the first sound by suddenly distending this latter cavity. The apex of the heart striking against the walls of the chest in the neighbourhood of the fifth and sixth ribs, communicated to the ear at this moment a shock (the heart's impulse). Immediately followed the second sound, produced by the onward current of blood through the aortic opening, propelled by the contraction of the ventricles. I observed, moreover, that the first sound did not exceed the space in which the impulse was felt, but that the second sound was audible in nearly the whole extent of the chest, which would tend to strengthen the theory I have advanced, inasmuch as the sound produced by the diastole of the ventricle would be circumscribed, whereas that produced by the systole would be diffused.—*Lancet, Jan.* 3, 1852, *p.* 8.

[Mr. CARTWRIGHT, in noticing the discrepancy which exists in the statements of authors as to the causes of the impulse and sounds of the heart, states his opinion that it proceeds from the fundamental error, that the impulse occurs during the systole, and that the difficulty can only be solved by returning to the theory formerly advocated by Dr. Corrigan and others. Mr. C. proceeds:]

In 1830, being then a student in Dublin, I heard of Dr. Corrigan's theory, that the impulse occurs during the diastole. On returning home I opened a rabbit, and not wishing to cause the animal unnecessary pain, I gave it a gentle rap on the head, sufficient to cause a depression of the bone. On opening the chest, I saw the heart beating quietly and regularly; after about half a minute, it became agitated, and during these agitated movements the heart appeared to be elongated, and its apex to be turned upwards, at the same time giving a kick, as it were—an expression much in favour with lecturers on anatomy twenty years ago. After about a minute the action of the heart became quieter, and the last movements resembled the first. A month ago, I examined two rabbits under the influence of chloroform: no irregular or spasmodic action occurred in either case; the heart appeared to dilate and contract in its natural manner. During its contraction the heart was evidently shortened and narrowed—that is, less in circumference; the apex was somewhat flattened, and appeared once to be actually drawn inwards; but during the diastole the apex shot out again with some force. These appearances are in perfect harmony with the anatomy of the muscular substance of the heart: the muscular substance of the left ventricle, for instance, may be to a considerable extent separated into two layers,—an internal, consisting of longitudinal fibres, running from the apex towards the basis, and an external, consisting of network fibres, running principally in a spiral direction. The contraction of the longitudinal fibres shortens the heart, and their consequent swelling fills up the space previously occupied by the blood driven out by their contraction; at the same time the spiral or circular fibres, by their contraction, force out a certain quantity of blood, and press in relative proportion the internal surfaces towards one another, so that a contracted heart is shorter and less in circumference than a heart dilated and full of blood; and as the heart lies in a sloping direction from above downwards and forwards, and with a part of its surface in close apposition to the inner surface of the chest, I must conclude that the impulse occurs during the diastole, when the heart is increased not only in length but also in circumference, and consequently presses closer to the inner surface of the chest than it does during the systole.

The ventricle is a sucking pump, and sucks the blood in; its dilatation is quite independent of the contraction of the auricle, which is proved by the following experiment: I cut the auricle through, the blood flowed into the chest, and the *ventricle dilated several times* afterwards. The heart also pulsates after it is cut

out and placed on the table. This active dilatation of the ventricle is caused by the elasticity or tone of the muscular fibres. What I understand by the elasticity or tone of the muscular fibres is the power by which they return to their natural position; thus if a muscle is stretched by an abscess underneath it, or by any other cause, on removing the cause, the muscle immediately returns to its natural position. This act is not contraction in its usual meaning. Also when a muscle, be it a voluntary or involuntary one, contracts, this occurs through the agency of a higher power; and when this power ceases to act, the fibres of the muscle do not remain relaxed in the same identical position, but return with a certain degree of force to their natural position. This is apparent in the biceps humeri, but still more so in the vastus internus and the gastrocnemii. This action—namely, the muscle returning to its natural position from a state of contraction or extension—depends on a principle resident in the fibres themselves, call it elasticity, tone, or by any other name; and it is through this principle that the dilatation of the heart takes place.

The semilunar valves are never pressed against the sides of the artery, otherwise they would be retained in that position by the continuous stream of blood; and it also appears to me, from observations on the hearts of animals in a state of spasmodic contraction after death, that certainly not more than half of its blood is expelled out of the left ventricle during each systole. With reference to the sounds of the heart, I may state, that the first is heard most distinctly at the apex; it appears to come towards the ear; whilst the second is acknowledged to arise at the semilunar valves, and to be carried along the aorta; this harmonizes completely with the diastolic theory; whilst the first sound is caused by the passage of blood through the auriculo-ventricular opening and the valve towards the apex; and the second sound is caused by the forcible propulsion of the blood through the semilunar valves. The bruits in disease of the heart are also more naturally and correctly explained by this theory, as may be proved by an analysis of the various treatises on auscultation, especially the admirable Treatise by Dr. Skoda, (fourth edition, 1850, Vienna,) a work no one can read without feeling the highest esteem for the candour and logical acumen of the learned professor. But it is not necessary to make vivisections or experiments on dead hearts; every man carries in his own breast evidence sufficient to prove that the impulse occurs during the diastole. I have myself made the following experiment several times: if a person lies in bed on the left side and listens attentively, he can hear the second sound most distinctly; should his heart palpitate or beat somewhat violently, he will feel at the same time synchronous with the second sound, a shock or concussion in the chest, extending even to the carotid arteries.

Each contraction of the ventricles causes one pulsation throughout the arterial system, before another contraction takes place; and as the arteries are elastic tubes, a certain interval or space of time, be it more or less, elapses, before the pulsatory wave, caused by the contraction of the left ventricle, reaches the extremities of the arterial system. The carotid artery, just before its bifurcation, may be considered as a middle point between the heart and the capillaries in the brain; consequently the pulse at that point ought to be intermediate between two contractions of the ventricle: and if the impulse of the heart is caused by its dilatation, it ought also to occur in the interval between two contractions of the ventricle: now the impulse of the heart and the pulse of the carotid are synchronous. This explanation also accounts for the impulse not being synchronous with, but immediately preceding, the pulse at the ankle. I conclude, therefore, that the impulse of the heart takes place during the diastole.—*Lancet, Dec.* 20, 1851, *p.* 578.

29.—*Statistical Report upon Cases of Disease of the Heart, occurring in St. George's Hospital, especially in relation to Rheumatism and Albuminuria.* By Dr. Barclay.—[At one of the meetings of the Royal Medical and Chirurgical Society, Dr. Barclay read a paper on this subject.]

The author first mentioned eight cases of acute rheumatism fatal during the rheumatic attack, among which he particularly called attention to a case of severe pericarditis, in which no friction sound was ever heard; and to a case in which no inflammation existed either in the heart or pericardium, in which a systolic murmur was distinctly heard. He next referred to sixteen cases of recent pericarditis, and

four in which it was not quite recent, occurring independent of acute rheumatism; two of which were traced to recent peritonitis, and one to recent pleurisy; a large proportion of the remainder were associated with albuminuria, but it seemed to him that in many of these instances, tumultuous action of the heart during life had very much to do with the traces of pericarditis found after death. He detailed one case in which this action of the heart seemed to be its only cause. Eleven cases were then mentioned in which recent lymph was found on the valves, and two in which it was not quite recent. Two were traced to malformation, and two to disease of the kidney; seven were accompanied by old disease. In six of these the old disease was probably due to bygone attacks of rheumatism, and he remarked it as a curious fact, that this condition was so much more frequently than any other associated with recent inflammatory action. Twenty-six cases of old pericarditis were then considered, including six of adherent pericardium, and three in which the adhesions were nearly universal; besides a great many not mentioned, in which white spots were found. He believed these cases indicated that universal adhesion is not the common result of rheumatic pericarditis; and considering the age of the patient at death, and the condition of the heart, that it is not the most favourable termination of the attack. More than half the cases which had at some period or other suffered from acute rheumatism indicated old pericarditis. Twenty cases of old valvular lesion were probably caused by acute rheumatism; twenty-six occurred in persons who had never suffered from rheumatism at all; sixteen were regarded as doubtful cases. Among the rheumatic cases there is a great preponderance of simultaneous lesion of aortic and mitral valves; next of the mitral alone, and scarcely any of the aortic alone. It seemed probable, in most of these cases, that during the first attack of rheumatism the mitral valves alone were inflamed, and at some future period also the aortic secondarily. Valvular lesion was found associated in twenty-two instances with diseased kidney and albuminous urine; in twenty-six instances the kidney was healthy, and in thirteen instances the evidences of disease were more or less obscure. The tricuspid valves were also affected in ten cases, and the pulmonic valves once. It was stated that among these cases disease of the aortic valves was most frequently found associated with hypertrophy, and in aortic regurgitation also with dilatation; disease of the mitral valves mostly with dilatation; adhesions of the pericardium more generally with dilatation; atheroma of the aorta and disease of the kidney were found accompanying a large majority of the cases of simple hypertrophy. In 141 cases of diseased kidney taken indiscriminately, hypertrophy occurred fifty-five times, and dilatation thirty-six times together or separately in sixty-three individuals, and in seventy-eight the heart was healthy. The author then detailed his observations in regard to rheumatism, made upon cases in the hospital which did not prove fatal. He found the heart diseased in forty-four per cent. of acute cases, in eleven per cent. of subacute, and four per cent. of chronic cases; in all the chronic cases, and many of the subacute, the individual had experienced a more severe attack of rheumatism at some former period; some of the acute cases were also examples of a second or subsequent attack. Taking those cases only which were admitted with a first attack of acute rheumatism, he found cardiac inflammation occur more frequently in females than in males, and in the largest proportion between ten and fifteen years of age, after which it gradually declined to forty. The majority of the acute cases are found under twenty-five years of age, and after that there is a much larger proportion of subacute cases. There was a larger proportion of males than females admitted with confirmed disease of the heart; but the difference is less striking if those of rheumatic origin alone be considered. At the earlier ages almost all cases of confirmed disease of the heart owed their origin to acute rheumatism, while at more advanced ages it almost ceased to be found among the causes.

[Dr. C. J. B. Williams, expressing his approbation of the paper, said:]

He (Dr. Williams) had seen many cases in which there were albumen in the urine, and hypertrophy of the heart, without any valvular disease of that organ. He was at first led to regard such cases as instances of organic disease of a very serious kind, but he had found that remedies applied for the relief of the kidney affection, removed the symptoms as well as the physical signs of the heart *disease to a greater* or less extent. In these cases there was at first a strong *heavy impulse of the heart*, a muffled systolic sound, and the usual pulse of

hypertrophy. Another variety of sound, too, which had not been noticed in the paper, was a reduplication of the first sound; as if the action of the two ventricles was not perfectly synchronous. These sounds and symptoms all implied organic disease of the heart; but he had found that as the medicines given for the kidney affection had reduced the quantity of albumen in the urine, the action of the heart became less, and the hypertrophy diminished. This circumstance tended to throw light on the production of the hypertrophy, which he thought was secondary to the kidney affection; and that it should be so, was easy to be understood; for if the kidney did not act properly, the heart would be abnormally excited by the diseased condition of the blood acting as an irritant, hypertrophy would be the result, the same as was observed in other muscles which had been under the influence of excitement. In none of these cases could he say that the heart and kidneys had been restored to health; but there had been such a diminution in the albuminuria and of the hypertrophy, that the patients usually got into a state of valetudinarian health. In some instances, however, these cases ran the usual course of those of organic disease of the heart. There was one other point in the paper to which he would briefly refer, and that was in relation to the production of an endocardial murmur in cases of rheumatism, in which, however, there was no endocarditis. He thought this statement must be taken with caution; for we could not suppose a murmur could exist in the heart without some physical cause. We must be well satisfied as to the manner in which the examination was made, and all the circumstances respecting it, before we arrived at the conclusion that such was the case. He (Dr. Williams) had known, in the early periods of rheumatism, an endocardial murmur, which after a time ceased. He had attributed the presence of this sound to rheumatic inflammation of some part of the lining membrane at the root of the artery, by which the action of the valves was interfered with. This might be removed, and no trace of disease be left behind; just as in patients who had been suffering from rheumatism, and had been cured, no trace of the disease might be discoverable.—*Lancet, Nov.* 22, 1851, *p.* 492.

DISEASES OF THE ORGANS OF RESPIRATION.

30.—ON RESPIRATION AS A PROCESS OF NUTRITION.

(From a review of Dr. Gairdner's work, in the 'Lancet.')

Dr. Gairdner repudiates entirely the school which treats of respiration as a mere means or process of defæcation. In this view, modern physiology fully bears him out,—though it is curious, in one of the very latest of the current theories of respiration propounded (Liebig's), to find the trail of the old idea still subsisting. In this theory, combination between the oxygen of the atmosphere and the carbon of the blood is not immediate or direct—the process is not begun and ended in the lungs; the oxygen has first to be absorbed by the blood, and combination to be afterwards effected in the general current of the circulation, the carbonic acid therein produced being finally eliminated in the act of expiration. This view has always appeared to us on many grounds untenable; the simple fact of the blood on the left side of the heart being from one to three degrees Fahrenheit higher in temperature than that on the right side, seems of itself to dispose of it. The additional degrees of heat could only have been obtained by the direct combustion of so much oxygen with so much hydro-carbonaceous matter in the lungs, and the production of commensurate volumes of watery vapour and carbonic acid gas. The end of respiration may be defined in few words to be, the accomplishment of certain changes in the constitution of the blood, these changes being necessarily accompanied by the evolution of a certain degree of heat, whereby the circulating fluid becomes at once the vehicle of vital warmth to all parts of the body, and the source of the several elements from which they are built up, repaired, or maintained in their integrity.

In respect to the blood itself, Dr. Gairdner, as we have seen, gives good reasons for concluding that oxygenation and fibrination are identical or convertible terms. As a general thesis, we may say that it is certainly by the action of the oxygen

inhaled, that the albumen of the blood is raised into the state of fibrine. But in the conversion thus effected, there must be something more than a direct chemical operation; for we cannot, by exposing albumen to the immediate action of oxygen, obtain fibrine. There must be some mean or medium between these two organic principles; and modern physiologists seem now pretty well agreed in regarding the blood-corpuscles as the efficient agent in securing the change of one into the other. The late lamented Dr. Simon, of Berlin, and our able countryman, Mr. Wharton Jones, independently of each other, and, as it appears, nearly contemporaneously, were the originators of this view. Neither Professor Henle nor Dr. Wagner, as Dr. Gairdner seems to think, had any share in announcing the idea; Henle's notions indeed are rather directly opposed to it, *his* theory being that the conversion of albumen into fibrine is an independent process; the liquor sanguinis or plasma, as a whole, he assumes as the cytoblastema or formative matrix, and existing as an act of the living organism. Now the liquor sanguinis, or compound fluid in which the blood-discs swim, is in an especial manner the cytoblastema in reference to these bodies; and it would have been illogical to maintain that any portion or element of a fluid, through and by which they themselves exist, is an effect of their existence. Wagner, we think, nowhere refers the formation of fibrine to the blood-corpuscles; we have at all events searched in vain through the writings of the illustrious Göttingen professor for such an opinion, though we find it ascribed to him by Dr. Carpenter in the last edition of his physiology; we must, therefore, conclude that this is the source of the error into which Dr. Gairdner has fallen upon this point. That the view of Simon and Wharton Jones in regard to the influence of the blood-corpuscles on fibrination is the correct one, however, everything that is yet known seems to proclaim. We have only, in fact, to connect the opinions and statements of the author we have in hand, on the part which oxygenation plays in the process, to have one of the very interesting questions in animal physics, satisfactorily and finally solved. The fibrinous element is never abundant, save among animals whose blood is rich in corpuscles, and in whom the respiratory process is actively carried on. In birds, where the respiration is of intensity enough to maintain the temperature of their bodies at some 6° or 8° Fah. higher than in man and the most perfect of mammiferous animals, the blood is also relatively the richest in fibrine; and as we descend in the scale of being, through amphibia and fishes, do we at once and coincidently find enfeebled respiration, diminished amount of blood-discs, and disappearance of fibrine. More than this, where in the higher grades of animal life there is rapid evolution of parts in conjunction with a less perfect action of the atmospheric air, as happens remarkably in the fœtus in utero, we find the albuminous element of the blood very abundant, and the corpuscles also present in extraordinary quantity; but, as if to satisfy us that a more immediate access of oxygen was indispensable to perfection in the chemistry of the animal body, little or no trace of fibrine. Still farther, and passing from health to disease, we observe that in inflammatory affections, which are characterised by the presence of an unusual amount of fibrine in the blood, there is a diminution—a coincident and rapid waste or disappearance of red globules. It is as if we had in this fact the demonstration presented to us of the connexion of one organic principle with another; precisely as in a mass of fermenting fluid, we see the sugar disappear, as alcohol and carbonic acid are evolved.

So satisfactory does the chain of evidence on this subject appear to us, that we hardly think it worth while to refer to any of the other theories of fibrination that have been propounded by physiologists. If we allude to the views of Dr. Carpenter on the matter, it is rather because they are the newest, than that they have more than good writing to recommend them. Dr. Carpenter believes that the white or colourless corpuscles of the blood are the source of the fibrine. Save the statement of Dr. Carpenter, that these colourless corpuscles are found in augmented numbers in the blood during febrile and inflammatory diseases, and that fibrine is found as a constituent of the blood in animals whose blood and blood-discs are alike without colour, there is, in truth, no body of facts in the whole round of animal physiology and pathology to countenance such an idea. The pathological statement has been oftener than once disputed by the most competent microscopical authorities; and that the colourless discs in the higher *animals have aught* especial to do with fibrination, is no legitimate inference

from the physiological fact above mentioned. Doubtless the white globules of the blood of molluscs and insects may suffice for the elaboration of the fibrine which is found in their circulating fluids; but to maintain that the rarely-occurring colourless discs of the blood of fishes, reptiles, birds, and mammals, are the sources of the fibrine, is really to assert an hypothesis improbable *primâ facie*, and unsupported by any body of collateral evidence. *Colour* is, indeed, such an *accident* in nature, that we can scarcely consent to regard it as a matter of real importance.

But the red globules of blood are not interesting only in their connexion with the production of fibrine. From the researches of Le Canu, confirmed by every subsequent observer, they appear to be the turning point, as it were, in the development of the vital fluid—the point at which assimilation is apt to pause. They attain their highest absolute as well as relative development in that state of highest health which verges on disease—in plethoric states, and diseases of plethora. On the other hand, they are most signally deficient in those shattered conditions of the body, in which life appears to be continued despite of the indifferent performance of the whole round of organic processes. Andral and Gavarret record one case of apoplexy, in which the globules were found in the excessive ratio of 175; and in men of sanguine constitution, Le Canu found them habitually in the high proportion of 136 in the 1000 parts. In dropsy, on the contrary—in chlorosis, diabetes, &c., the globules are constantly found in no larger proportion than 68, 59, and even 55, in the 1000 parts.

In contrast with this very remarkable difference in the globules, it is curious to observe the wonderful steadiness of the albumen. Whatever the proportions of the other organic elements, the albumen in all the various states of health and disease in which the blood has been examined, has still been found very constantly within, or but little beyond, the limits of 70 and 80 in 1000 parts. It would seem as if nature had found it necessary to have at hand something like an unvarying stock of the raw material, out of which all the other principles of the blood might be fashioned, according to the varying states and requirements of the organism. One particularly interesting point in connexion with this view is found in the fact that relatively the albumen and the blood-globules appear to alternate with one another in their several proportions—if the globules fall off in amount, the albumen is increased; and *vice versâ*, when the globules abound the albumen is diminished. To have a demonstration of this matter—for Dr. Gairdner is obviously a man who takes nothing for granted that he can submit to the test of experiment—our author had three rabbits abundantly fed for many days, and their blood examined. The results obtained are comprised in this table.

	Albumen.	Globules.	Fibrine.
1	50·20	97·40	2·10 } in 1000
2	49·	80·50	1·75 } parts.
3	48·20	75·43	1·96 }

Though consisting of but few elements, this table is, nevertheless, full of interesting facts, and affords ground for important inference. The first rabbit, we are informed, was allowed its liberty in a room, whilst the two others were closely confined in a box. Now a glance suffices to show the influence of exercise, and the free play of the lungs, on the constitution of the blood. Whilst there is remarkable accordance in the proportion of the albumen, the other two more highly elaborated principles, the globules and the fibrine, differ considerably. The mean results of the analysis Nos. 2 and 3, contrasted with the analysis No. 1, stand thus:—

	Albumen.	Globules.	Fibrine.
1	50·20	97·40	2·10 } in 1000
2 and 3	48·60	77·96	1·85 } parts.

The terms of comparison contrasted in this way, the resemblances and differences meet the eye most prominently.

Surely there is in these results some positive addition to our knowledge? And we here presume boldly to assert that progress *has* been made in the theory of fibrination, in the teeth of a statement to the contrary, which we are surprised to find in the pages of a contemporary,* whilst noticing the work we have ourselves in hand. The conclusion of the reviewer referred to, indeed, cannot be held worthy of high consideration, inasmuch as he has not given any reasons for his judgment. If Dr. Gairdner be in error, he has at least given good reasons for the opinions he holds, and has shown how much countenance they receive, not from his own views and experiments only, but from parallel conclusions arrived at by some of the most eminent physiologists of the age.

The changes in the organic constitution of the blood just illustrated, are undoubtedly, as our author observes, of much importance in the history of constitutional disease.

"If the nutrient matters of the food (he says) be carried forward to this point (the point of globulation), and here, instead of further elimination, be deflected to form lower products than the fibrine and gelatine, which would serve the healthy purposes of the body, we must look here both for our explanation of disease and our guide in the choice of a remedy. We may now well understand the quotation I have made from Dr. Prout, at p. 6. Though I do not believe that scrofula takes its origin in a gouty state of habit, but that, like gout, it is to be referred to an original congenital constitution, an idiosyncrasy of the individual, yet its outward manifestations, its allied disease, tubercle, and many other tumours, may be traced to this source. All these growths abound in albuminous principles. Their progress is favoured by that which lowers the health and disturbs nutrition. The food which ought to proceed to the nourishment of the great organs and moving powers of the body, is interrupted in its course, and diverted to engender morbid processes or add to mal-organisations already begun. In some individuals, extraordinary obesity is the result, and where subcutaneous fat only is created, it often serves as a relief to a plethoric habit. In others, low hydro-carbonaceous products show themselves in the urine, in the form of sugar or urates. We can thus readily enough account for the most striking phenomena of scrofula, phthisis, diabetes, gout, &c., according to the proclivity of the individual to one or other form of disease; nor will it appear wonderful to the philosophic physician, that maladies of aspects so strikingly different, should yet own an origin in some respects common.

"These are not creations of the fancy. They are exemplified in the daily experience of every physician. What observation so common as the origin of scrofula in an impoverished condition of health? Is it not observed, that a low quality of food tends to its increase, and a generous diet to its extinction? Does it not infest the close and pestilential manufactory, all low, marshy, and mephitic places? Is it not cured by pure, good air, as well as good food? Do we not turn our scrofulous patients out of hospitals for fear of the consequence to their health? In a pure and wholesome atmosphere the chest is thoroughly expanded, and the necessary changes in the blood well effected.

"In gout the very same influences prevail. Only the tendencies of the constitution being different, the plethoric or superfluous albuminous matters show themselves in different local manifestations. The treatment, too, has various points of resemblance. Both are aided by whatever promotes the real nutrition of the body, and obviates stagnation and load in the vessels."

He sums up in these words:—

"I have now gone through the whole of this subject of the chemical and physical constitution of the blood, with a view to prove that rest and repletion lead necessarily to accumulation of globules; that aeration is the source of the fibrine; that by exercise the fibrine is carried forward to the tissues; that by exercise, air, and moderation in diet conjoined, constitutional disease, and particularly gout, may be avoided and cured; that without them it is vain to hope for anything more than a respite from suffering for a greater or shorter

* The British and Foreign Medical Review for October last.

period, or even only a suspension of the most acute symptoms of disease. These observations are so consonant with the observation of all men, learned and unlearned, of every age and every country, that they will, I feel persuaded, meet with ready belief."

The nature of gout must indeed be sought for in those difficult and hidden regions where organization begins. But this makes the study of the disease singularly interesting. It has hitherto been prosecuted far too empirically. We hail with joy every sign of a higher and more wholesome study of our art, and while our respectable physicians maintain their place among the sciences, we shall disquiet ourselves less about the conduct of those unworthy members of our profession who seek their own advantage in its degradation.—*Lancet, Dec.* 13, 1851, *p.* 557.

31.—ON BRONCHITIS.

By Dr. R. B. Todd, F.R.S., Physician to King's College Hospital.

In a first attack of bronchitis, the lungs are not altered in structure, or the chest in form; but after repeated attacks both are affected in a manner which I shall presently describe to you.

It is remarkable that in these cases we always find that the expiratory sound is more or less prolonged; in some instances a long and somewhat sharp wheeze forms the sound of expiration, which at times is as long and even longer than the inspiratory sound.

In the normal state of respiration you know that the chief sound is that caused by the ingress of air into the lungs, or the inspiratory sound, and that the sound of expiration is so slight that it is scarcely appreciable, and, indeed, in most instances, is inaudible. This is doubtless owing chiefly to the extreme rapidity with which the lung, by its elastic reaction, expels the air; and partly, perhaps, it may be due to the great softness of the pulmonary tissue, which makes it an imperfect conductor of sound. When, however, the bronchial membrane is thickened by inflammation, and the tubes narrowed by the same cause, and their canals impeded by mucus, the air passes out of the lung with difficulty, in consequence of the obstacle to its exit which it thus meets with. Moreover, the pulmonary tissue around the tubes is increased in density, and becomes, in virtue of that increase, a better conductor of sound, so that not only is the exit of the air from the lung much retarded, but its outward passage is rendered much more audible.

Such are the changes and physical signs which we find in the early attacks of bronchitis. But when there have been repeated attacks of this disease—and especially where these acute attacks have supervened upon a continuous chronic bronchitic state (if I may be allowed to coin such an adjective)—the lungs undergo very serious changes, which interfere greatly with their functional integrity and activity. These changes are of various kinds: sometimes one or more bronchial tubes are obliterated (as first, I believe, pointed out by Dr. Stokes), and the pulmonary lobule or lobules to which they lead collapse from the absence of their usual distending medium, the air, and become more or less wasted. The adjacent tubes and their corresponding air-cells dilate to receive more air, just as the tubes of one lung would if the other were compressed by fluid, but probably to a much greater degree; and thus we may have in the same lung at parts a collapse and an atrophy of portions of the lung, and at other points expansion and permanent dilatation of the air-tubes and air-cells.

But there are other and more potent causes in operation to promote the dilatation of the air-cells and tubes. These are the disturbed state of breathing caused by the bronchial irritation, and more especially the difficulty of expiration, and the mischief done to the tissues of the bronchial apparatus by the repeated attacks of inflammation. Thus the bronchial irritation gives rise to a more or less asthmatic state, in which the act of inspiration is performed with considerable force, and that in a state of lung which is ill-suited to resist the pressure of the in-rushing air. The muscular fibres of the bronchial tubes must, by the repeated attacks of inflammation of the mucous membrane, be more or less weakened. Now the most probable office of these fibres is to regulate the admission of air into the lung,

and thereby to protect its delicate tissues against undue pressure, just as the muscular fibres of the arteries regulate the flow of blood into them, and to a certain extent antagonise the heart's force. Hence in an enfeebled state of this muscular apparatus the bronchial tubes will yield under the force of the inspired air, and become more or less dilated; and an undue quantity of air will rush in most abundantly at those parts where the muscles are weakest, and therefore afford least resistance.

Again, when the air has accumulated in the lung, it is with difficulty expelled. There are direct obstacles to its outward passage in the altered condition of the mucous membrane of the tubes, and in the accumulation of secretion in them. Moreover, the expelling force is in great part due to the reaction of the elastic tissue, which enters largely into the formation of bronchial tubes, and which invests the lobules of the lung. But the undue stretching to which this tissue has been subjected, not only from the forced inspiration, but from the detention of the air within the lungs, and the accumulation of mucus in the tubes, must, as the disease advances, more or less impair its elastic power, and therefore weaken the force which takes the most direct and the largest share in the process of expiration. Thus the longer the duration of the disease, and the more frequent the attacks, the more serious will be the evils which follow in its train.

You may readily gather from what I have already said what are the alterations in the lung which chronic bronchitis tends to produce. They are—first, the immediate changes, and, secondly, the remote ones. The immediate changes are those which affect the mucous membrane and muscular fibres of the bronchial tubes, as well as the tubes themselves; such as inflammation, thickening, altered secretions —perhaps even ulceration—and also more or less dilatation of the tubes. The remote changes are a still further dilatation of the tubes—a dilatation of the air-cells; and when that dilatation goes beyond a certain point, a stretching, and even a rupture of many of the bands of elastic tissue which are found in the lobules. This stretching of the bronchial passages and cells gives rise to a corresponding change in the air-cells, which exercises a very marked influence upon the capillary vessels of the lung, which, so far as I know, was first pointed out by Mr. Rainey, of St. Thomas's Hospital. The expansion of the air-cells causes an extension of the meshes of the capillary net-work distributed upon and within them; and the rupture of many of the intersecting bands of fibrous tissue causes obliteration of their blood-vessels. Thus the capillary system of the lung becomes diminished in its capacity, and thus is explained the fact long known, that emphysematous lungs are apt to be pale, and to look as if they contained but little blood.

Now, the state to which the lung is thus brought by a long continuance of chronic bronchitis, is that which we call *emphysema*, in which there is more or less dilatation of a greater or less number of air-cells, and a consequent diminution in the area of the capillary system belonging to them.

Chronic bronchitis, however, is not the sole cause of emphysema, although certainly the most frequent. That state of lung will follow repeated attacks of asthma: and it may be caused by great and prolonged efforts; and there are those who believe that it may arise even in the absence of any such exciting causes, in persons who have a certain constitutional weakness of the lungs, which may be inherited.

In both the acute and chronic forms of bronchitis one of the most valuable remedies is counter-irritation. This I employ very freely in these cases, not so much by blisters as by turpentine or mustard, and there is this great advantage in this mode of counter-irritation, that you can apply it frequently and at short intervals, and moreover it is immediate in its effects, whereas a blister takes several hours to produce vesication, and it cannot be speedily reapplied. Dry cupping is also a useful form of counter-irritation, and very applicable to such cases as I have mentioned.

Generally speaking, patients labouring under bronchitis, and especially those who have had many attacks, are not very tolerant of a depleting or depressing treatment. General bleeding by venesection is, in many instances, highly dangerous; topical bleeding is borne better; when tried, only a small quantity of blood should be taken.

The medicines most applicable to these cases are those which produce a free

diaphoresis, and expectorants, sometimes sedatives. When the expectoration is viscid, and sticks to the tubes so as to make it difficult of dislodgment, great benefit often results from the cautious use of tartarized antimony in small doses; but this must be used only for a very short time, as it tends to produce a profuse watery expectoration, and very much to weaken the patient: as soon, therefore, as the very viscid character of the expectoration is overcome, it should be given up.

When you wish to promote expectoration without causing any undue increase in the quantity of secretion, you will find nothing better than ammonia. In bronchial catarrhs, if there be fever, it may be given freely with the liquor ammoniæ acetatis, and you thus get a copious diaphoresis also. I am also in the habit of using the chloric ether pretty freely in bronchial attacks, either alone or conjoined with ammonia. It is a valuable stimulating expectorant, and has some sedative influence likewise: if not given in too large a dose it is an agreeable medicine to take, and forms a pleasant ingredient for a cough mixture. The decoction of the polygala senega is much lauded for its influence on bronchial affections; I have given it very freely, and, except for its unpleasant taste, can find no fault with it, nor can I bestow upon it any very strong encomiums.

With the use of sedatives you require caution, especially with opium. Conium, hyoscyamus, hop, &c., are well borne on the whole; but nothing relieves irritable cough so effectually as opium; yet when there is much bronchial congestion, you will beware of using it too freely, as it unquestionably tends to increase that, and to endanger the life of the patient. On the other hand, when expectoration is free or too profuse, a moderate dose of opium often exercises the most beneficial influence, procures sleep, moderates expectoration, and relieves the cough. The reputation of the old paregoric elixir, modernised into compound tincture of camphor, is likely to last even through these days of scepticism.

In the more advanced stages, and especially if there be sweats, tonics are useful, and sometimes astringents containing tannin, or even the tannic or gallic acid.—*Med. Gazette, Nov.* 21, 1851, *p.* 874.

32.—*On the Catarrhal Pneumonia and Lobar Pneumonia of Children.* By MM. Trousseau and Lasegue.—Catarrhal (or lobular) pneumonia is a disease as distinct from simple (lobar), as variola is from erythema. This is seen in their respective mortality. Of twenty children who have been admitted into the hospital clinique, suffering from *simple pneumonia*, in six months all have recovered; of nearly thirty who were attacked with *catarrhal pneumonia*, not one survived. Most of the first class of cases exhibited an excessive degree of acuteness, which burnt out like a fire of straw; while several of the second, notwithstanding their fatal termination, commenced with very mild symptoms.

Simple pneumonia hardly ever affects a child below two years of age, and rarely those of two or three, but becomes of more and more frequent occurrence as the child approaches adolescence. Its cause and symptoms resemble those of the adult, with some modifications. After twenty-four or thirty-six hours, the souffle and bronchophony can alone be heard; the crepitant râle, which is often observed in the adult when the patient coughs, even when much souffle is present, is hardly ever heard in the child. So afterwards, from day to day, without the crepitation of resolution, the souffle disappears, leaving only a feeble respiration. The progress of the disease is also more rapid than in the adult. In the mild form of the disease, recovery takes place rapidly, and in large proportion; but in its grave form, many cases are lost by any mode of treatment. M. Trousseau generally bleeds the child, gives it an emetic of sulphate of copper, and then a mixture, containing Kermes mineral and extract of digitalis.

Catarrhal pneumonia commences with a catarrh, which rapidly extends to the small bronchi, and then we hear numerous and small sub-crepitant *râles* disseminated over both lungs, and especially posteriorly. These *râles* may persist for four, six, eight, or fifteen days, without any *souffle* becoming manifest; but sooner or later we hear a *souffle*, the resonance of the cries or the voice, or at least a prolonged respiratory murmur. While these latter sounds, common to simple and catarrhal pneumonia, are thus manifesting themselves, we find by the subcrepitant

râles, that the capillary catarrh is still persisting in the rest of the lung. The disease has extended from the mucous membrane to the parenchyma of the organ. Febrile action is less than in ordinary pneumonia, being predominant at some portions of the day, and entirely ceasing at others; and these alternations of better and worse may continue for fifteen, twenty, or thirty days; the disease being originally a pulmonary catarrh, and partaking of the obstinacy and uncertainty of catarrhal complaints. As more and more of the parenchyma becomes implicated, the fever becomes more continuous and intense, and the respiration more difficult, until the children die exhausted. In other cases, in which the bronchial phlegmasia was very intense from the first, and the lung became rapidly invaded over a great extent, death takes place with rapidity. The progress of the disease has usually been more rapidly fatal, when it has succeeded to measles, chronic disease of the skin, or laryngitis. All means of treatment that have been tried have proved impotent.

These two affections may be compared, *exceptis excipiendis*, with erysipelas and phlegmon. Erysipelas traverses the surface, like the catarrh; and when it persists too long, it induces ulcerations of the skin, furuncles, and circumscribed subcutaneous abscess, just as the capillary catarrh induces suppuration of the lobules, little abscesses of the lungs, and circumscribed pneumonias. Simple pneumonia, on the other hand, progresses like simple phlegmon, violent in its febrile reaction, but terminating abruptly and rapidly.

It must not be supposed, from what has been said, that catarrhal pneumonia is almost invariably fatal. Although this is the case amidst the miasmata of an hospital, which exert effects at once so terrible and so difficult to avert, it is not so in private practice. In this, one-half the patients may be cured, by repeated vomiting, flying blisters, antimonials, and digitalis; but how terrible are the ravages of a disease, which, under the most favourable circumstances, kills ono-half its subjects!—*L'Union Médicale*, 1851, *No.* 115.—*Brit. and For. Med. Chir. Review*, *Jan.* 1852, *p.* 261.

33.—REMARKS ON PHTHISIS.

By Dr. C. A. Wunderlich, Tübingen.

The influence of climate on the development of phthisis is a subject on which much has been written, but regarding which little has been satisfactorily demonstrated. Bennoiston de Chateauneuf has calculated that of one thousand soldiers dying in the north of France, eighty-five were tuberculous; while of an equal number in the central parts and the south, seventy-three and eighty-two were thus affected. In Marseilles, one-fourth of the population is carried off by phthisis. It is common in the West Indian islands, in Madeira, in Rio Janeiro, in New Zealand, in Nice, Florence, Naples (where, according to Journé, three deaths in every seven result from phthisis), in Malta, Spain, Portugal, Calcutta, and Madras. Hence, no climate appears to afford an exemption from this malady. Wunderlich seems to incline to the belief that there is an antagonism between intermittent fever and phthisis; and hence that marsh-lands do, to a certain degree, afford a protection against this disease.

The following are the *local processes in the respiratory organs* which may give rise to tubercular deposits:

Frequent attacks of acute bronchial catarrh seem to predispose towards phthisis, or at all events to hasten the eruption of its symptoms: so, also, do epidemic catarrh and pertussis; while, on the other hand, chronic bronchial catarrhs seem to keep off pulmonary tuberculosis.

Pulmonary congestion is very favourable to the development of tubercle, especially when it frequently recurs and affects the upper lobes.

Hemorrhage may give rise to the formation of tubercle in a secondary manner, by the retention of coagula.

Pneumonia is a very frequent cause of tuberculosis, the remains of the non-absorbed pneumonic infiltration being readily metamorphosed into tubercular matter.

Emphysema, on the other hand, has a tendency to exclude tuberculosis in the lungs, or at all events to check its extension.

Pleuritis with adhesions predisposes to tuberculosis, the metamorphosis commencing in the plastic exudation, and from thence extending to the lung. Compression of the lung, to a certain degree, but not altogether, excludes the development of tubercle.

The forms of tuberculosis described by Wunderlich are—

1. Miliary granulations.
2. Crude tubercles, or tubercular nodules.
3. Tuberculous infiltration.
4. Tuberculous exudation in the smaller bronchial tubes.

"The further metamorphoses of pulmonary tubercle, whether occurring as granulations, nodules, or infiltration, are—

"1. *Softening*.—This sometimes commences at the centre, sometimes at some other point, and sometimes at several points at once. The mass, which was previously firm and dry, and of an almost chalky white, or very slightly pale yellow tint, begins to change to a deeper yellow, and to become soft and pulpy, till it is finally converted into a fluid containing fragments of solid tubercle, and which on a microscopic examination is found to present the elements of tubercle and a few granular corpuscles (pus-corpuscles). Thus the tubercle is converted into an abscess, sometimes only at particular spots, as in infiltration, but sometimes over its whole extent.

"Simultaneously with this softening process, there is generally a further extension of the tubercular deposit in the surrounding part; and as this additional tubercular matter also softens and becomes dissolved, the fluid contents unite with those in the first abscess, and the abscess thus enlarged finally opens (most commonly) into a bronchus, but occasionally (when the disease has been very widely extended) into the pleura, and thus discharges its contents. In this way abscesses become converted into open caverns. The tuberculous cavity varies in appearance, according as this process has been gradual or rapid. In the former case, the tubercular matter at the spot is usually completely fused, and the cavity, which can then thoroughly empty itself, has smooth and tolerably regular walls. The surrounding pulmonary tissue presents a bluish tint, is lax or callous (callöse), exhibits no cellular structure, and is strewed over with isolated tubercles. If, on the other hand, the progress of the abscess has been rapid, and it has opened very rapidly, we seldom find that the whole of the tubercular mass is dissolved. The cavity is then generally irregular, opening in various directions, and containing undissolved, jagged masses of tubercle. The adjacent tissue is in a state of infiltration. Many of these cavities sometimes communicate, and we may find a whole lung perforated with them. It is seldom that a large tuberculous cavity has a simple form; even when its walls are smooth, it is usually sinuous and presents compartments, partly because it is made up of the union of several cavities, and partly because some portions of tissue not thoroughly destroyed, run across or project into the cavity in the form of rafters, ledges, cords, and bridges. These are generally composed of an obliterated bloodvessel and some atrophied and compressed lung-substance, which in this state have resisted further destruction. It is rarely that any blood passes through such a vessel; and it is only when the disintegrating process has gone on very rapidly, that the walls of open vessels are destroyed, and that blood can in this manner escape. The contents of an abscess, previously to its opening, are usually yellow, greasy, and sometimes a little reddish. After an opening has ensued, but before the cavern has perfectly emptied itself, the contents sometimes present an external similarity to pus, or they may be more diluted and mixed with flocculi, and be either of a dirty red, a brown, a gray, or even a blackish tinge. In a cavern of this kind, we not unfrequently find free and detached, but not perfectly destroyed, portions of lung. When the contents are completely discharged, the walls of the cavern remain the seat of an ichoro-purulent secretion, and are usually invested with a greasy pseudo-membrane; these walls may become the seat of gangrene.

"If an abscess breaks into the pleural cavity, its fluid contents are effused into the pleural sac, unless impeded by the presence of strong adhesions; and as there

is usually a communication between the cavern and a bronchial tube, air enters with each inspiratory movement into the pleural sac. Hence there is developed a severe inflammation of the pleura with a plastic, purulent, and often ichorous exudation; and we have bulging of the side of the chest, and compression of the lung, in consequence of the entrance of the air. Perforation of the pleura is, however, comparatively rare, because the firmest adhesions usually occur at the spot where the tuberculous deposits make their way to the pleura.

"2. *Atrophy* may occur among the miliary granulations, converting them into hard, very small, bluish gray, or black nodules, which are incapable of any further development, and which finally gradually disappear. Whether the larger tubercular deposits can undergo a similar metamorphosis is problematical.

"Various pathological peculiarities may be referred to the resorption and obsolescence of tubercle. Boudet ('Rech. sur la guér. nat. et spont. de la phthisie pulm., 1843,' p. 11) regards the change which tubercles undergo into an oval or elliptic form, as the commencement of resorption. Fournet ('Rech. clin.' II. t. 929, &c.) explains the transformation of the lungs at their apices into a black and often shrivelled mass, which is sometimes hard, and intersected by cellular and fibrous cords, and is covered by thickened and wrinkled pleura—a change of no rare occurrence, as any one with much experience in *post-mortem* examinations can testify—as representing the remains or cicatrices of old tubercles which have been resorbed in their crude state. In examining the bodies of persons who have died from other diseases, (once in a case of cancer of the stomach, and on another occasion in an old drunkard, who died from pulmonary infarctus,) I have sometimes found distinct chalky concretions in spots which have undergone this change, which seem to me to strengthen the view that these puckerings and shrivellings *may* depend on old resorbed tubercles; but whether in other cases they may not originate in a different manner, as, for instance, from obliteration of the terminal portions of the bronchial tubes, from shrivelled pneumonic infiltration, or even from insidious atelectasis, is a point which I cannot decide.

"3. *Cretification* is the ordinary mode in which crude tubercle and tuberculous infiltration are rendered innocuous to the organism. Cretification occurs in much the same manner as the atheromatous and chalky degeneration of plastic exudations. The conditions necessary for cretification are, doubtless, poverty of the blood, deficiency of the vital powers, and little motion of the surrounding parts; and it appears to begin to occur when softening commences. When the tubercle has become half-softened, the process stops, and there is a deposition of a preponderating quantity of salts (some of which are soluble, as the phosphate, hydrochlorate, and sulphate of soda, while others are insoluble, as the phosphate and carbonate of lime) and an abundance of crystals of cholesterin. The mass gradually dries, and there is left a chalky residue, which at first is triturable and sandy, but finally is studded with sharp spicula of the hardness of bone, and which neither exerts any disturbing influence on the adjacent lung-substance, nor on the organism in general. It is very probable that even this fragment is at length disintegrated and disappears. This fortunate mode of termination is, however, often frustrated by the circumstance, that while some of the tubercular deposits which are favourably situated for this process are healing by cretification, the disease is advancing in other parts of the lungs.

"4. In some very rare cases, the tubercle becomes *encysted* by a tough wall formed by the reaction that is established in the adjacent tissue. The tubercle is either in a state of crudity, or is calcified.

"5. The *cicatrization* of caverns can only occur when their contents are thoroughly discharged. The walls acquire firmness and toughness, and are invested either with a mucous-membrane-like coating, or with a partially ossified callus. One or more bronchial tubes open into the cavity. A cavern of this nature may remain for a long time without undergoing any apparent change, and may during this period secrete pus either scantily or copiously. Or its aperture may close, while the investing membrane gradually approximates to a serous coat, the contents become more aqueous, and the cavern itself becomes converted into a serous cyst. Or finally, the cavern may gradually diminish, till it at length disappears, and there remains in its place nothing but a cartilaginous, fibrous, or cellular cicatrix. All these modes of termination are comparatively rare, and only

occur when the tubercles are few and scattered, and when the tuberculous dyscrasia has become extinct."—*Brit. and For. Med. Chir. Review, Jan.* 1852, *p.* 112.

34.—ON THE PATHOLOGY AND GENERAL TREATMENT OF PHTHISIS PULMONALIS.

By Professor BENNETT, Edinburgh.

Many observing physicians have not failed to notice, that phthisis pulmonalis is ushered in with a bad and capricious appetite, a furred or morbidly clean tongue, unusual acidity of the stomach and alimentary canal, anorexia, constipation alternating with diarrhœa, and a variety of symptoms denominated dyspeptic, or referable to a deranged state of the primæ viæ. Moreover, it can scarcely be denied that, in the great majority of cases, these are the symptoms which accompany phthisis throughout its progress, becoming more and more violent towards its termination. Now, as the nutritive properties of the blood are entirely dependent on a proper assimilation of food, and as this assimilation must be interfered with in the morbid conditions of the alimentary canal, the continuance of such conditions necessarily induces an impoverished state of that fluid, and imperfect growth of the tissues. Moreover, when, under such circumstances, exudations occur, it has been shown by the histologist that they do not exhibit any tendency to perfect cell formations, but that corpuscles are produced, which form slowly, and slowly break down, causing softening, and the production of ulceration, which becomes more and more extensive as the amount of the exudation increases.

An observation of the circumstances which precede the disease, or its so-called causes, clearly indicates imperfect digestion and assimilation as its true origin. Thus phthisis is decidedly a disorder of childhood and youth—that is, a period of life when nutrition is directed to building up the tissues of the body. Diminish the proper quantity of food taken by a healthy man, tubercular diseases are not induced; but if this be attempted with children or young persons, they are a most common result. It has been supposed that hereditary predisposition, a vitiated atmosphere, changeable temperature, certain occupations, humidity, particular localities, absence of light, and so on, predispose to phthisis. Very frequently several of these are found united, so that it is difficult to ascertain the influence of each. When they so operate, however, they invariably produce, in the first place, more or less disorder of the nutritive functions, and are associated with dyspepsia, or other signs of mal-assimilation of food.

From a study of the symptoms, causes, morbid anatomy, and histology of phthisis pulmonalis, we are therefore led to the conclusion, that it is a disease of the primary digestion, causing,—1st, impoverishment of the blood; 2nd, local exudations into the lung, which present the characters of tubercular exudation; and, 3rd, owing to the successive formation and softening of these, and the ulcerations which follow in the pulmonary or other tissues, the destructive results which distinguish it. Further observation shows, that circumstances which remove the mal-assimilation of food frequently check further tubercular exudations, while those which previously existed become abortive, and that occasionally more extensive excavations in the pulmonary tissue may, owing to like circumstances, heal up and cicatrize.

A healthy nutrition of the body cannot proceed without a proper admixture of albuminous and oleaginous elements. This may be inferred from the physiological experiments of Tiedemann and Gmelin, Leuret and Lassaigne, Magendie, and others; from an observation of the constituents of milk, the natural food of young mammiferous animals; from a knowledge of the contents of the egg, which constitute the source from which the tissues of oviparous animals are formed before the shell is broken; and from all that we know of the principles contained in the food of adult animals. The researches of chemists, such as those of Prout, Liebig, and others, point to the same generalisation, when they assert that carbonised and nitrogenised, or, as they are now called, respiratory and sanguineous food, are necessary to carry on nutrition, inasmuch as oil is a type of the one, and albumen of the other. The chemical theory is imperfect, however, because it does not point out,

how these elements form the tissues; for it is not every form of carbonised or of albuminous food that is nutritious, but only such kinds of them as are convertible into oil and albumen.

The reason of this was first pointed out by Dr. Ascherson, of Berlin, in 1840, and made known by me to the profession in this country in 1841. I have since endeavoured to show that the elementary molecules formed of a particle of oil, surrounded by a layer of albumen, which are produced, as he described, by rubbing oil and albumen together, are not developed directly into blood-globules and other tissues, as he supposed, but must first pass through a series of transformations,—a knowledge of which is highly important, not only to a comprehension of nutrition generally, but especially to that anormal condition of it which occurs in phthisis. Thus the successive changes which occur for the purposes of assimilation in the healthy economy may be shortly enumerated as follows:—1st. Introduction into the stomach and alimentary canal of organic matter. 2nd. Its transformation by the process of digestion into albuminous and oily compounds: this process is chemical. 3rd. The imbibition of these through the mucous membrane in a fluid state, and their union in the termini of the villi and lacteals to form elementary granules and nuclei: this process is physical. 4th. The transformation of these, first, into chyle corpuscles, and secondly, into those of blood: which is a vital process. It is from this fluid, still further elaborated in numerous ways, that the nutritive materials of the tissues are derived, so that it must be evident, if the first steps of the process are improperly performed, the subsequent ones must also be interfered with. Hence we can readily comprehend how an improper quantity or quality of food, by diminishing the number of the elementary nutritive molecules, must impede nutrition.

The peculiarity of phthisis, however, is, that an excess of acidity exists in the alimentary canal, whereby the albuminous constituents of the food are rendered easily soluble, whilst the alkaline secretions of the saliva and of the pancreatic juice are more than neutralised, and rendered incapable either of transforming the carbonaceous constituents of vegetable food into oil, or of so preparing fatty matters introduced into the system, as will render them easily assimilable. In consequence, more albuminous than fatty matters enter the blood, and the necessary waste of structure is supplied by the absorption of the adipose tissues of the body. Hence the emaciation which characterises the disease. In the meanwhile, the lungs become especially liable to local congestions, leading to exudation of an albuminous kind: which is tubercle. This, in its turn, being deficient in the necessary proportion of fatty matter, elementary molecules are not formed so as to constitute nuclei capable of further development into cells,—they therefore remain abortive, and constitute tubercle corpuscles. Thus the local disease is added to the constitutional disorder, and that compound affection is induced, which we call phthisis pulmonalis—consisting of symptoms attributable partly to the alimentary canal, and partly to the pulmonary organs.

To improve the faulty nutrition which originates and keeps up the disease, it is of all things important, therefore, to cause a larger quantity of fatty matter to be assimilated. A mere increase in the amount, or even quality of the food, will often accomplish this. The treatment practised, some years ago, by Dr. Stewart, of Erskine, which consisted in freely administering beef-steaks and porter, and causing exercise to be taken in the open air, excited considerable attention from its success. I have been informed, that in some parts of America the cure consists in living on the bone marrow of the buffalo, and that the consumptive patient gets so strong in this way, that he is at length able to hunt down the animal on the prairies. All kinds of food rich in fat, will not unfrequently produce the same effects, and hence the value long attributed to milk, especially ass's milk—the produce of the dairy, as cream and butter, fat bacon, caviar, &c.

But, in order that such substances should be digested and assimilated, the powers of the stomach and alimentary canal must not have undergone any great diminution. In most cases it will be found that the patient is unable to tolerate such kind of food, and that it either lies undigested in the stomach, or is sooner or later vomited. Under these circumstances, the animal oils themselves are directly indicated, *by giving* which, we save the digestive apparatus, as it were, the trouble of *manufacturing or separating* them from the food. By giving considerable quanti-

ties of oil directly, a large proportion of it is at once assimilated, and is rendered capable of entering into combination with the albumen, and thereby forming those elementary molecules so necessary for the formation of a healthy chyle. Such, it appears to me, is the rationale of the good effects of cod-liver oil.

Since I introduced this substance to the notice of the profession as a remedy for phthisis, in 1841, I have continually prescribed it in hospital, dispensary, and private practice. I need not, perhaps, say, that I have given it in a very large number of cases and have observed its effects in all the stages of the disease, and under almost every circumstance of age, sex, and condition. I have had the most extensive opportunities of examining the bodies of those who have died after taking it in considerable quantities, and am still observing the cases of many persons who may be said to have owed their lives to its employment. Further, I have carefully watched the progress it has made in the good opinion of the professional public, and perused all that has been published regarding it in the literature of this and other countries. It were certainly easy for me, therefore, to write at great length on this subject; but I do not see that anything of utility could be added to what I have already published. The following is a summary of my views regarding cod-liver oil as a remedy for phthisis:—

1. Cod-liver oil is, as M. Taufflied pointed out, an *analeptic*, and is indicated in all cases of anormal nutrition dependent on want of assimilation of fatty matter.

2. It is readily digestible under circumstances where no other kind of animal food can be taken in sufficient quantity to furnish the tissues with a proper amount of fatty material.

3. It operates by combining with the excess of albuminous constituents of the chyme, and forming in the villi and terminal lacteals those elementary molecules of which the chyle is originally composed.

4. Its effects in phthisis are to nourish the body, which increases in bulk and in vigour; to check fresh exudations of tubercular matter, and to diminish the cough, expectoration, and perspiration.

5. The common dose for an adult is a table-spoonful three times a-day, which may often be increased to four, or even six, with advantage. When the stomach is irritable, however, the dose to commence with should be a tea or dessert-spoonful.

6. The kind of oil is of little importance therapeutically. The pure kinds are most agreeable to the palate; but the brown coarser kinds have long been used with advantage, and may still be employed with confidence whenever cheapness is an object.

7. I have never observed its employment to induce pneumonia, or fatty disease of the liver or kidney, however long continued, although such complications of phthisis are also exceedingly frequent.

Whilst I consider such to be the general pathology and treatment required for phthisis, which should never be lost sight of, you will be greatly mistaken if you suppose that the indications stated can always be carried out. In practice, the great difficulties to be overcome, are the numerous complications of phthisis, but, above all, that excessive derangement of the alimentary canal, which is alike the cause of the disease and the obstacle to cure.—*Monthly Journal of Med. Science, Jan.* 1852, *p.* 56.

35.—ON THE EARLY SIGNS OF CONSUMPTION.

By Dr. Theophilus Thompson, F.R.S., &c.

[It is a most important object to be able to detect the first approach of this deadly disease. It may be stated that one of the earliest signs which can be traced by auscultation is a modification of the expiratory murmur, consisting in an apparent prolongation, usually accompanied with an increase of coarseness.]

The natural elasticity of the lungs is essential to soft and uniform expiration. When considerable consolidation is produced in their texture by tubercular or pneumonic deposit, bronchial expiration is produced; but between the healthy state and decided consolidation there are various intermediate conditions. When the pulmonary cells, as seen under the microscope, are only slightly thickened,

and the glairy, greyish deposit, studded with little bright cells, characteristic of phthisical disease at an early period, is beginning to permeate the structure, bronchial expiration is not induced, but the diminished contractility of the cells, interrupted passage of air, and increased power of conducting sound, are sufficient to render the expiratory murmur more durable, coarse, and audible. In pursuing this investigation, be careful not to confound the inspiratory and expiratory movements with the inspiratory and expiratory murmurs. The duration of the two movements is nearly, if not exactly equal. In the natural state the inspiratory murmur occupies the whole time of inspiration, but the expiratory murmur, at least to ordinary ears, only a fourth of the time of expiration, the remaining part of the expiratory movement being accomplished in silence. I believe the expiratory murmur follows the inspiratory immediately without a pause. With the progress of phthisis the duration of the inspiratory murmur usually lessens materially, though not necessarily in proportion to the prolongation of the expiratory; and some practice is necessary in order to acquire an aptitude in determining how much of the alteration depends on diminution of the duration of the inspiratory murmur, and how much on extension of the expiratory. You will find much assistance in estimating the relative duration of these sounds, by adopting a plan suggested to me by Dr. Sibson—namely, that of counting the number of strokes which can be given by beating time with the finger during the presence of each murmur respectively. The expiratory murmur, as disease advances, may gradually increase, until, instead of occupying, as in the natural state, a fourth of the period of healthy inspiration, it may even come to exceed in duration the inspiratory murmur.

You will occasionally find it stated, even in writings of some authority, that prolonged expiratory murmur is a sign of doubtful value, and not to be relied on; but when reasons are given for this assertion, you will find them unsatisfactory. If no symptom of disease were to be regarded which did not require to be accepted with some qualification, and interpreted with discrimination, the science of diagnosis would dwindle into childishness. What, then, are the cautions to be observed in attempting to deduce conclusions from the sign under consideration? You will best learn them by examples. In the man, B. H., now before us, you find the expiratory murmur equal in duration to the inspiratory, over nearly the whole chest; but the sound on percussion is for the most part clearer than natural, and the diaphragmatic ribs rather sink in than advance during inspiration. This patient has not an aspect nor a pulse characteristic of phthisis. His countenance is slightly livid, as though from imperfectly oxygenated blood; he has never had hæmoptysis. You see the pulsation of his heart in the epigastrium. Such a case you would never mistake for one of phthisis. You recognise it at once as one of extensive emphysema, and the prolonged expiratory murmur thence derives a ready explanation.

Take another patient in whom prolonged expiratory murmur is heard extensively, and indifferently at the lower and upper parts of the chest, but associated with sonorous and sibilant rhonchi. This is a case of chronic bronchitis. There is no circumstance to lead you to apprehend consumption. Again, you are probably aware that consolidation of lung in any part, from pneumonic or other deposit, may produce bronchial breathing, and the same cause, existing in a slighter degree, may induce prolonged expiratory murmur; but you will almost always find, in the constitutional circumstances, the history, the expectoration, and the other physical signs, enough to guide you to the correct interpretation. Let me contrast such conditions with those in which the expiratory murmur is modified by tubercular disease. In the patient whom I now introduce, P. D., you find, adopting the means formerly described, that the expiratory murmur at the apex of the left lung is equal to the inspiratory, each occupying the time required for five beats with the finger, and that the interval of silence is equal to two. Under the right clavicle the expiratory murmur might be represented by three. In other parts of the chest, expiration is not attended by any audible sound. There is no bronchial rhonchus, and the situation and degree of the phenomenon lead you to suspect phthisis. The movement of the chest is natural, and there has been no decided hæmoptysis; but I think you will be able to distinguish a slight degree of dulness when you strike the left clavicle, as compared with the right, and you

will observe an irritable or quivering action of the intercostal muscles produced by a smart blow, which is worthy of notice as by no means uncommon in phthisical individuals. This patient has had a cough for six months; his expectoration is mucous, but, under the microscope, a few blood-globules may be detected. His height is five feet four inches; vital capacity by spirometer, 155 cubic inches, not quite a fourth less than the average for his height, and he weighs ten stone six pounds, having never, he says, exceeded eleven stone when in good health. These particulars, while they strengthen the conclusion to which you are led by the degree and place of the prolonged expiratory murmur, also serve to impress the value of the sign, by showing that the disease is at a somewhat early period.

In the next patient, W. U., the sign being only on the right side, were it not considerable, would be inconclusive, on account of the greater audibleness of the respiration on this side in the natural state; but it is so much prolonged, in this patient, as to exceed the inspiratory in length, the proportions being, four for the inspiratory murmur, five for the expiratory, and three for the interval of quiet. Furthermore, there is a little dry crepitation at the apex of the right lung. He has had occasional hæmoptysis, has declined in weight fifteen pounds during the last two years, and has almost lost his voice. The aphonia depending probably on a relaxed condition of the laryngeal membrane, we have applied medicines locally, by means of a slightly curved glass tube, as recommended by Trousseau and Belloe—if, indeed, we do not owe the original suggestion to Aretæus. In the first instance we blew in alum, and subsequently nitrate of silver, mixed with twelve times its weight of sugar, just at the moment of inspiration. This mode of introducing medicinal agents into the windpipe is not, however, so easy and effectual as that adopted by Dr. Horace Green. This probang, having a soft, globular piece of sponge, capable of absorbing about twenty minims of water, very carefully fastened to the extremity, if *very slightly* curved, can be readily introduced under the epiglottis when passed steadily and firmly downwards, close by the root of the tongue. I usually employ a solution of two scruples of the crystals of nitrate of silver to the ounce of water, according to the recommendation of Dr. Green; and in relaxed and some other diseased conditions of the mucous membrane, the benefit derived from this measure is often considerable, although you would not be so unreasonable as to rely on such a measure alone, unassisted by judicious attention to the management of the general health.

One more case—that of J. C.—I bring before you, the sign in question being confined to the right side, with reference to the objection that prolonged expiratory murmur on the right side is consistent with health. In this patient, the expiratory murmur near the right apex is as five, and the inspiratory as four. On the left side, the expiratory murmur is not above a third of the inspiratory. This difference is too great to be attributable to natural condition. You possibly detect slight dulness on percussion on the right side. He has had no hæmoptysis; but the pulse, which in the sitting posture is 80, is only 84 when he stands. He is nearly five feet seven inches high, and weighs ten stone two pounds and a half—a fair average for a man of his height. He has long had cough, but his constitution is scarcely at all affected, and but for his cough he would probably not have applied for medical relief. The disease is evidently at an early period; the symptoms, however, as far as they go, are definite, and the sign under our special consideration takes the most prominent place.

During the time of my attendance on the out-patients of this institution, I made this symptom an object of particular notice, and among 2000 consumptive patients, it proved to be the most remarkable of the physical signs in 288,—those cases presenting bronchial complications being excluded. Hæmoptysis had occurred in 91 of these cases; that is, in 31 per cent.,—a proportion calculated to confirm my opinion of the significancy of the prolonged murmur; and at the same time sufficiently below the average frequency of hæmoptysis in the first stage of phthisis, to support the assumption that the prolonged expiratory murmur takes precedence of other characteristic signs which are commonly assumed as requisite for the proof of consumptive disease.

With a view to the correct appreciation of the sign under our consideration, keep

in mind the situation, degree, persistency, and simplicity in which it is presented. Prolonged expiratory murmur slight in degree, if heard only on the right side, is inconclusive, but is far more significant if confined to the left. The more limited the space over which it is heard, the more does it suggest phthisis as the cause. The persistency of the sign for a considerable period, and unattended with symptoms of bronchitis, emphysema, or pneumonia, is a proof of some permanent obstacle to the free exit of the air, and in a majority of instances this obstruction is of a tubercular character. In cases depending on pulmonary congestion, the expiration, after cupping, and other appropriate treatment, usually resumes its natural character; but excluding this and other complications before noticed, I have not recorded any instance in which this sign, once fully established, ever ceased to be obvious.

Although, in many instances, no specific complaint was made, except of debility, and although, in this respect, improvement occurred under suitable treatment, it was yet common for the murmur to increase in duration, and deviate more and more from its natural character, whilst, sooner or later, dull percussion, bronchophony, hurried breathing, quick pulse, emaciation, and night perspirations, too often occurring in succession, afforded affecting testimony to the correctness of the first diagnosis.

A disturbed rhythm of murmurs, when established, I believe to be an unnatural condition, and the greater frequency of its detection on the right side may, I conceive, be readily explained; for if the respiratory sounds be naturally rather louder on the right side than on the left, the more delicate indications of pulmonary obstruction should first be detected in that direction. It would follow that in doubtful cases of apprehended phthisis, the absence of any changes in the expiratory murmur at the upper part of the right lung, would be a strong presumptive evidence of freedom from the disease. It is true that in certain individuals some degree of febrile action attends tubercular cachexy, before any local signs exist of tubercular deposit; but it has repeatedly occurred to me, when hereditary phthisis has manifested itself in a family, to be able, on examining the respiration of the surviving members, to prognosticate the approach of phthisis from this sign alone, in the absence of any other suspicious circumstance, either local or general. When the expiratory murmur is altered, in consequence of emphysema, or bronchitis, the extensive diffusion of the sign, and the other concomitant circumstances, will usually suggest a correct interpretation; and if we are careful to separate such sources of fallacy, I cannot but believe that the sign under review will prove no useless refinement, but one well deserving of careful attention, and perhaps the most early, significant, and conclusive of the evidences of incipient phthisis. It is no valid objection that the detection of the symptom requires close attention, since the object is to trace the first appreciable inroads of an insidious disease. To sum up the conclusions to which the instances and statements now adduced conduct us, I would observe that when the expiratory murmur is heard extensively, or on both sides, unassociated with bronchitis, emphysema, or condensed lungs, there is great reason to fear, not only that the disease is phthisical, but that it will make rapid progress. When the change of murmur is limited to a small portion of lung, and the general condition of the patient is favourable, the evidence of phthisical disease is fully as conclusive, but there is ground to hope that by careful regulation of diet, by securing exercise in the open air, by promoting healthy nutrition, and administering suitable remedies, such as iodine, iron, solution of potash, and cod-liver oil, more decided mischief may for a time be averted. I have reason to think that, under such circumstances, some years may occasionally elapse before softening takes place; and I cannot but believe that when this particular sign under consideration is more generally sought for in suspicious cases of phthisical tendency, the average duration of pulmonary consumption in persons possessing means to avail themselves of necessary comforts, will be found considerably to exceed the period commonly assumed.

The symptom which it is the object of this lecture to illustrate, must be considered to take precedence of various other rather early indications, which have been incidentally noticed in former lectures, such as an unduly diffused impulse of the *heart, especially* on the right side of the chest, or a murmur in the second *intercostal space to the left* of the sternum, which probably owes its source to the pul-

monary artery. In many instances the prolonged murmur precedes even cough.—*Lancet, Nov.* 22, 1851, *p.* 479.

36.—*On Jerking or Wavy Inspiration.* By Dr. Theophilus Thompson, F.R.S., &c.—[The symptom here referred to is a modification of the inspiratory sound designated "Inspiration entrecoupée," by Laennec; "Inspiration saccadée," by Fournet; and by some English writers, "jerking Inspiration." This term, however, is liable to objections, as it conveys to the mind the idea of the spasmodic jerk in the breathing produced by asthma, hysterical conditions, or other nervous disturbance.]

It is difficult verbally to describe this peculiar inspiration. To some observers it might seem appropriate to compare it to the movement of a cog-wheel, two or three cogs advancing in each act of inspiration. To others, the idea is better conveyed by representing inspiration as affected by a succession of waves, rather than a continuous current. Under this idea, I propose to term the peculiarity, wavy inspiration. The inspiratory sound, instead of being equably sustained from its commencement to its termination, is divided into several parts. Sometimes it is more feeble than natural; at other times rather harsh, but not necessarily altered in character.

An examination of my notes, recording the history of about a hundred patients in whom wavy inspiration was a principal symptom, has convinced me that there are certain varieties of this kind of inspiration with which it is important to be familiar. One of these varieties probably depends on obstruction to respiration, produced by pleuritic adhesion. This variety of the sign occurs in nearly equal proportion on the right and the left side; it is, I think, increased by pressing the stethoscope firmly over the part; it is often attended with pleuritic friction-sound, and is usually very distinct.

Another variety is apparently associated with rheumatic conditions. The wavy inspiration thus produced is usually high in tone, often rather widely diffused, and variable in situation, usually accompanied by pain in the part. Small doses of colchicum are useful in the treatment of this affection, and the use of lemon-juice is very appropriate. This latter remedy has also appeared to me of service in some instances of intercurrent pleurisy in the consumptive.

A third variety occasionally accompanies bronchial affections, and the cause may be detected by the co-existence of rhonchi and other symptoms characteristic of bronchitis. Interrupted inspiration, however, according to my observation, in a great majority of instances has no necessary relation to either of those conditions. The sign, in a very large proportion of cases, is limited to the left side, (contrasting remarkably, in this respect, with prolonged expiratory murmur;) and this fact is inconsistent with the idea of its necessary dependence either on pleurisy or phthisis. The usual situation of the sound is near the apex of the lungs in front; but occasionally I have observed it at the posterior part of the chest, where, indeed, it may occasionally be present without attracting notice.

Having devoted some attention to this inquiry, I must acknowledge that although wavy inspiration, when dependent on pleurisy, bronchial affection, or rheumatism, may disappear, I have not satisfied myself of its removal (unless superseded by more serious symptoms) in any instance unconnected with the conditions which I have specified. To what cause then can the production of so persistent a phenomenon be attributed? Its persistency points to some organic change, whilst its frequently stationary character encourages the belief that such change is not necessarily tubercular. The sensation conveyed to the ear suggests the idea of some obstruction in the pulmonary tissue, yielding at gradual intervals to the admission of air.

Sufficient opportunities have not occurred for conclusively testing any hypothesis, but the explanation which would appear best calculated to meet the conditions would assume the existence of an exudation less albuminous than tubercle, less fibrinous than the usual product of common inflammation about the walls of the small bronchial tubes, and the interstices of the pulmonary substance, calculated to impair the elasticity of the surrounding structure.

The frequent existence of such an exudation is not a matter of speculation; it is,

indeed, described by Rokitansky as not infrequent. We may suppose that such morbid product of low inflammation may sometimes be sufficiently plastic to become organized, and to constitute hypertrophy of the cells and lobules, without involving any serious pulmonary disorganization or constitutional disturbance. In other instances it may be associated with the germs of true tubercle, or become a favourable nidus for tuberculous deposit. When, indeed, distinct manifestations of phthisis appear in patients exhibiting this sign, the disease usually advances with marked rapidity. We may form an approximative idea of the proportionate degree of probability of such a course, from the relative frequency of the occurrence of hæmoptysis. We may assume this symptom to occur in half the cases of phthisis in the first stage, as determined by recognised signs. I have formerly stated that it occurs in about a third of those cases in which prolonged expiratory murmur was the principal physical sign; and in the cases which I have analyzed, chiefly characterized by wavy inspiration, hæmoptysis occurred in about a fifth.

The conclusions to which I am conducted in pursuing this inquiry are—

1st. That wavy inspiration may be occasionally observed in cases of bronchial inflammation, pleurisy, and rheumatism; but that the most common variety is unconnected with those affections.

2ndly. That the variety most frequently observed (and distinguishable from the other varieties) may occur at various parts of the chest, but most frequently at the upper part of the left lung.

3rdly. That the symptom rarely disappears, excepting when superseded by definite signs of phthisis, but often continues for years without the supervention of that disease.

4thly. That when phthisis does succeed, there is reason to apprehend rapidity in its progress.

Assuming these views to be correct, this symptom cannot be regarded as conclusive evidence of the presence of tubercular disease, but it does not the less retain its interest and value. Although such patients may be considered on the verge of phthisis, it is gratifying to feel authorized to assure them that there is no evidence of tubercular disease, and that under favourable circumstances we may hope to avert any more serious impairment of health. The indications of treatment are, to promote healthy nutrition, to correct hepatic congestion as far as possible, to obviate depressing mental emotion, to adjust clothing, and select climate, so as to secure as far as possible, without unseasonable exposure, much easy exercise in the open air. Iodide of potassium is often an appropriate remedy, and in many instances, iodide of iron; and counter-irritation is sometimes useful; but we must always remember that, although the disease is tending to localize itself in the lungs, yet the source is elsewhere.—*Lancet, Nov.* 29, 1851, *p.* 501.

37.—*On Hepatic Congestion attending Cases of Incipient Phthisis.* By Dr. Theophilus Thompson, F.R.S.—On the whole, I know no medicines calculated to obviate hepatic congestion, and to correct the condition of blood which may be assumed to characterize cases of incipient phthisis, better than a combination of chalybeates with saline aperients. For those who are able to obtain the mineral waters, whether at the natural spring or factitiously prepared, a course of the Kesselbrunnen of Ems is appropriate; or if there be tendency to hæmoptysis, the Saratoga Congress Spring of America may be given in preference. When these remedies are not accessible, a substitute may be obtained by administering two grains of sulphate of iron, a drachm of sulphate of soda, a scruple of carbonate of soda, and ten grains of dinner salt, in half a pint or a pint of warm water every morning. Exercise in the open air, the shower bath, and friction of the skin, are important auxiliaries in the treatment.—*Lancet, Dec.* 6, 1851, *p.* 524.

38.—*On the Effect of Moral Depression in the Production of Phthisis.* By Dr. Theophilus Thompson, F.R.S.—The medical statistics of our prisons afford *striking evidences* of the influence of depressing emotion. In a very interesting

communication, (published in the 'Transactions of the Royal Medical and Chirurgical Society, vol. xxviii.') Dr. Baly has collected together particulars regarding the health of prisoners in various parts of this country, and in America; and one of the most important conclusions which those particulars serve to establish, is the remarkable prevalence of consumption in those establishments, under every variety of climate, diet, and general regulation. This greater liability to phthisis, than exists among the ordinary population of these countries, is obviously independent of locality, and can only be referred to some common cause. At the Millbank penitentiary, the mortality from consumption is four times the average in the general population; and it is worthy of remark, that in those who fall victims to the unfavourable influence, the average period for the full development of its effect is about three years.

The interesting question will probably occur to you whether the causes to which I have referred as tending to induce consumption are equally influential with both sexes. The statistics at our command indicate considerable difference in this respect. In support of this assertion, let me call your attention to the fact, that although in the country at large many more women than men fall victims to consumption, yet in London the mortality of women from this disease is much less than that of men, and their comparative immunity is specially observable in married life; the proportion of married women compared with married men dying of this disease, above the age of twenty-five, appearing from our hospital report to be in the proportion of three to five. The physical circumstances of the two sexes in a great city being for the most part similar, we must seek in moral causes for the explanation of this remarkable circumstance; and such are easily assigned. On the one hand, we see man, struggling in this crowded metropolis for the support of his family, being beset with the wasting influences of anxious care; whilst on the other hand, we find woman, endowed with more faith and patience (provided she receive her just share of consideration and tenderness), fulfilling cheerfully the duties of the day, and not so anxiously regarding the morrow. On consulting the returns of the registrar-general, you will find confirmation of this view, in the remarkable fact that phthisical mortality in some provincial cities, as well as in the metropolis, is low for women, but that in the counties, the proportion is in a very high degree to the disadvantage of the female. An important exception to this rule, as applied to the large provincial cities, is furnished by Leeds, where the excess of female mortality from phthisis is equal to that which obtains in the kingdom generally. I know not how these differences are to be explained, except by some peculiarities of moral condition; and in seeking for criteria by which to measure the moral condition of women in different districts, it occurred to me to examine the registrar-general's returns of the number of illegitimate children born respectively in each of the places which I had entered as illustrating the contrasted mortality of men and women from the particular disease under consideration.

Proportion of Deaths from Phthisis to every 200,000 of Population.			Proportion per cent. of Illegitimate Children to Births
	Men.	Women.	
London - - - -	·455	·377	3·2
Liverpool and West Derby -	·595	·571	3·6
Manchester and Salford -	·549	·548	5·8
Leeds - - - - -	·440	·477	6·0
England and Wales - -	·378	·408	7·0
Paris, according to Benoiston.	·208	·408	28·0

The above table is deduced from the return for 1837, giving the order of increase in the proportion of female mortality, the per-centage of deaths being in each instance calculated from 200,000 of the population. I have added to the table a column for the per-centage of illegitimate children, in relation to the whole number of births, and although this return is for a different year, the relative proportion in different places for several successive years, where it is recorded, sufficiently cor-

responds to exclude any important fallacy in this direction. I do not intend to imply merely that the mothers of illegitimate children are peculiarly prone to phthisis, but rather that in the districts where the proportion is high, such a system of morals may be assumed to prevail as must extensively expose the female population to special causes of moral disquietude.

The return from Paris, even allowing some deduction for its large foundling hospitals, is remarkably high, and the comparative female mortality of that city is singularly predominant. You will agree with me that in any district most notorious for the number of children born out of wedlock, the women may be assumed to be least happy in condition, most exposed to hope deferred, unrequited affection, bitter ingratitude, and all the causes which tend to wither the heart of those whose lives may be said to ebb and flow with their emotions.

Whilst, on the one hand, I cannot doubt the influence of mental depression in conducing to the establishment of phthisis, on the other hand I cannot resist the force of evidence that cheerful impressions have considerable power in retarding the progress of the disease.—*Lancet, Dec.* 13, 1851, *p.* 548.

39.—*Treatment of Cough in Phthisis.* By Dr. Theophilus Thompson, F.R.S. —Perhaps there is no symptom of which consumptive patients complain so urgently as of cough, and there is scarcely any which so often baffles our efforts for its relief. This subject is far too wide to be treated systematically in a single lecture, for the severity of the cough is doubtless modified by a variety of circumstances, such as the amount of bronchial irritation, the quantity and kind of expectoration, the constitutional irritability, and the condition of the skin. If any degree of bronchial inflammation be present, small doses of antimony are indicated, and in some instances the application of a few leeches over the windpipe affords much relief. In allaying cough in irritable subjects, much advantage is often derived from the administration of four-minim doses of tincture of aconite in spermaceti mixture; and counter-irritation may often be resorted to with advantage. When the urgency of the cough has more relation to the character of the expectoration than to nervous irritability, lemon-juice has sometimes seemed to me to be a useful remedy. In other similar cases the solution of potash does good, especially in combination with squill; although this latter remedy, in cases complicated with any degree of bronchial inflammation, would probably aggravate the symptoms.

Anodynes, such as hemlock, henbane, Indian hemp, are occasionally useful, and there can be no doubt of the value of hydrocyanic acid. A draught, containing an eighth of a grain of cyanide of potassium, an ounce of aniseed water, and some syrup of lemons, is perhaps more trustworthy than prussic acid in the usual form of administration. In a great number of instances, however, we are obliged to place our chief reliance on opium, or on some of the salts of morphia. An agreeable linctus may be composed of an ounce of conserve of roses, half an ounce of lemon-juice, half an ounce of syrup of poppies; or a drachm of tincture of opium and a drachm of diluted sulphuric acid may be mixed with an ounce and a half of treacle or honey, and a teaspoonful given occasionally.

You have had an opportunity of observing the relief often derived by inhalation: an ounce and a half of the strobiles of hop in a pint of hot water sometimes proves very soothing. Some of our patients have derived still greater advantage from inhaling two grains of extract of opium, by means of the apparatus introduced by Dr. Snow. On the whole, however, no remedy has acted so promptly and satisfactorily in allaying cough, as the inhalation of fifteen or twenty minims of chloroform.—*Lancet, Dec.* 20, 1851, *p.* 571.

40.—*On Cod-Liver Oil in Phthisis.* By Dr. Walshe.—The conclusions at which Dr. Walshe has arrived with regard to the use of cod-liver oil in the treatment of phthisis, are as follows:

"1. That it more rapidly and effectually induces improvement in the general and *local symptoms than any* other known agent. 2. That its power of *curing* the

disease is undetermined;—I mean here, by 'curing' the disease, its power of causing, along with suspension of progress, such change in the organism generally, as shall render the lungs less prone to subsequent outbreaks of tubercles, than after suspension occurring under other agencies. 3. That the mean amount of permanency of the good effects of the oil is undetermined. 4. That it relatively produces more marked effects in the third, than in the previous stages. Opinions the most diverse have been held on this point; M. Taufflied taught that it had little or no effect on phthisis, if at all advanced; M. Péreyra *reduced the size of cavities in a few weeks* by its administration. 5. That it increases weight in favourable cases with singular speed, and out of all proportion with the actual quantity taken;—that hence it must in some unknown way save waste, and render food more readily assimilable. 6. That it sometimes fails to increase weight. 7. That in the great majority of cases, where it fails to increase weight, it does little good in other ways. 8. That it does not relieve dyspnœa out of proportion with other symptoms. 9. That the effects traceable to the oil in the most favourable cases are increase of weight, suspension of colliquative sweats, improved appetite, diminished cough and expectoration, cessation of sickness with cough, and gradual disappearance of active physical signs. 10. That in some cases it cannot be taken, either because it disagrees with the stomach, impairing the appetite (without itself obviously nourishing), and causing nausea, or because it produces diarrhœa. 11. That in the former case it may be made palatable by association with a mineral acid; and in the latter prevented from affecting the bowels by combination with astringents. 12. That intra-thoracic inflammations and hæmoptysis are contra-indications to its use, but only temporarily so. I have repeatedly given the oil within a day or two of the cessation of hæmoptysis, without any return taking place. 13. Diarrhœa, if depending on chronic peritonitis, or secretive change, or small ulcerations in the ileum, is no contra-indication to the use of the oil; even the profuse diarrhœa caused by extensive ulceration of the large bowel is not made worse by it. 14. That the good effects of the oil are *cæteris paribus* directly as the youth of those using it,—a singular fact, and which probably may one day (when the textural peculiarities of youth and age are better understood) aid in giving a clue to its mode of action."—*London Journal of Medicine, Dec.* 1851, *p.* 1132.

41.—*Suggested Improvements in the Form of the Stethoscope.* By Dr. Stanhope T. Speer, Cheltenham.—The difficulty not unfrequently experienced in examining with the ordinary stethoscope the chest of a person in whom, whether from great emaciation, or any other cause, the ribs are very prominent, has of late struck me forcibly, and has been more prominently brought before my notice from happening to have at one time four cases of thoracic affection, in all of which there was great difficulty in making an accurate examination of the chest with the ordinary stethoscope, which in such cases (unless some intervening body be placed between it and the ribs) usually allows air to get within its cavity. Bearing this in mind, it struck me that an instrument might be made, having the ordinary bell-shaped mouth, modified in such a manner as to enclose a sufficiently large column of air, and at the same time to fit accurately into the spaces between the ribs. For this purpose, I made a drawing of a stethoscope, and one of which I have succeeded in making of gutta percha. It differs from that usually in use, by the orifice, instead of being circular, having the shape of an almond, the bore becoming gradually more and more circular up to the ear-piece. Its application requires rather more care than the circular stethoscope, to prevent being tilted up; but, if carefully applied, it will be found to fit into the intercostal spaces with great accuracy, and thus facilitate the examination in a remarkable manner.

Since devising the above, I have been surprised to find that a similar idea had occurred simultaneously with regard to an Italian physician of Palermo, in Sicily—Dr. Bianchi, who has forwarded a very similar one to the Academy of Medicine of Paris, and upon which Professor Piorry has been commissioned to report. As being considered worthy of notice by the first medical body in the world, I have thought it right to put forward my own suggestion, which occurred to me upwards of a month ago, and of which I then made a drawing, without the

slightest idea that such a modification of the instrument had ever before been thought of.

I may add, that for this form of stethoscope soft cedar is the best material, as it is, indeed, for every other.—*Med. Gazette, Dec.* 26, 1851, *p.* 1099.

42.—*On Emphysema of the Lung.* By Professor BENNETT.—[In some cases in the clinical reports, Dr. Bennett comments as follows upon the emphysema occurring in a case of chronic bronchitis:]

Emphysema is characterised anatomically by a permanent enlargement of the air-vesicles of the lung. These may frequently be seen through the pleura, with an ordinary lens, like groups of minute pearls. One or more of these may break into each other, and produce larger distensions, say, the size of a millet seed, and this process of enlargement may go on, by the breaking down of the intervening partitions, until every size of emphysematous cavity may be formed from the minute dimension alluded to, to that of a large orange. The walls of such cavities remain permanently open, having lost their elasticity; they are also evidently atrophied, and the paleness of the parts affected proves that the capillaries have been so compressed as to be either obliterated or impervious to the passage of blood.

In order to account for emphysema, numerous theories have been advanced, of which I shall allude to only the first and last. Laennec supposed that the fine bronchial tubes became rigid and more or less impervious from swelling of their lining membranes or impaction of mucus. He conceived that inspiration was a more powerful action than expiration, so that while air could be drawn through the obstructions it could not be breathed out. In consequence, it accumulated in the ultimate pulmonary vesicles, became expanded by heat, and so acted mechanically as a dilator, distending them from within, and causing them to enlarge more and more according to the duration of the disease, and extent of the respiratory efforts. Dr. Gairdner, however, has lately pointed out that expiration is a much more powerful act than inspiration, and that there is never any difficulty in causing expulsion of air. It is the inspiration which is laborious in all bronchitic cases, and, as has been previously stated, when the tubes are obstructed, so far are the air-cells beyond them from being dilated that they are in truth collapsed. Emphysema, then, does not occur in the vesicles connected with obstructed tubes, but in those healthy ones which are adjacent. When the lungs are in a normal state, the column of air presses equally on all the tubes and vesicles, but when one portion of the obstruction is collapsed or otherwise diminished in bulk, then the neighbouring portion is over-expanded, so as to occupy the space previously filled by the former. Hence why emphysema occurs not only as a result of bronchitis, but of chronic phthisis, or any other disease which causes contraction and hypertrophy of the pulmonary fibrous tissue. This theory is certainly most consistent with known facts, and may be therefore considered as not only probable, but as tolerably well established.—*Monthly Journal of Med. Science, Dec.* 1851, *p.* 551.

43.—*On Pulmonary Abscess.*—By ROBERT MOLLOY, Esq.—[In speaking of the use of iodide of potassium in the treatment of abscess of the lungs, Mr. Molloy observes]

Of the beneficial effects of iodide of potassium in most cases of affection of the lungs, I can speak with great confidence, given of course, as it should be, when the period of acute inflammation has subsided. The complaints in which I have found it productive of most benefit are—broncho-pneumonia, pneumo-bronchitis, chronic bronchitis, pituitous catarrh, and what is commonly termed *dry* asthma—viz., deficient secretion of the bronchial mucous membrane. Of its value in these affections, and why beneficial results should be looked for, I have never yet seen any reasonable explanation. The following has been supplied by my own observation, and I beg to offer it, as most probably correct. It is based upon the undoubted fact, that iodide of potassium is a general stimulant of the mucous *membranes, producing* a rapid secretion in the gastro-pulmonary tract and its

glands, acting more tardily upon the kidneys, as is shown by the increased flow of urine during its use, while its absorbent powers, however great they may be, are too remote to require present consideration. In a state of health, purification of the blood takes place, not in the air-cells of the lungs alone, but in the whole of the respiratory tract, from the mouth to the minutest bronchial tubes; in fact, in the whole of the mucous membrane over the surface of which the air passes. But in many of these diseases, mechanical obstructions arise; the mucus thrown out on the surface of the bronchi is viscid, glairy, and tenacious, not permitting the passage of air through it. Hence the blood in the membrane beneath ceases to be decarbonized, and a condition exists similar to what would be produced by sheathing the tubes with honey, a strong solution of gum arabic, or, a better example still, of nitrate of silver. If iodide of potassium be given under these circumstances, a thin aqueous saline secretion speedily takes place, which, by permitting aeration of the subjacent membrane, gives rise to almost instantaneous relief. I need hardly allude to its producing similar effects, although differently explainable, in dry asthma or in chronic bronchitis, when the globular masses of mucus act mechanically in preventing the due purification of the blood; but I can state from repeated observation, that the exhibition of this remedy in these instances will at once change the character and consistence of the sputa, and afford an amount of relief which we may vainly expect from any other medicine. My reliance upon it therefore is very great. The only instance I can call to mind where it was productive of mischief was in the case of œdema of the lungs, with hydrothorax and anasarca; here its use was followed on two occasions by secretion of dark-coloured massive expectoration, mixed with clots of venous blood, and was in consequence withdrawn.—*Lancet, Jan.* 24, 1852, *p.* 92.

44.—*On Spasmodic Asthma.*—Under this head, Dr. WALSHE states the following remarkable and important fact, with regard to the hygienic treatment of the disease. "Change of air is most important; but the kind of change that shall prove most beneficial can only be learned by experience. Some sufferers lose their paroxysms south of the olive line, others are easiest in a cold atmosphere; moisture, the bane of some, greatly mitigates the disease in others. The air of towns suits some, that of the country others; the clear suburban air of London is infinitely more noxious to some asthmatic persons than the foul atmosphere of the worst cleansed and most densely peopled localities of the metropolis; occasionally an individual will be found who is tortured with asthma in one room of a house, free from it in others; and this without any distinct explanation being found in the aspect, the drainage, or any other known condition."—*London Journal of Medicine, Dec.* 1851, *p.* 1133.

45.—*Treatment of Asphyxia Infantum.*—Dr. TOTT states, that he has often succeeded in restoring life in the *asphyxia asthenica infantum* after the failure of the usual means, by causing a person to stand on a table, and pour cold water from a tea-kettle on to the pit of the stomach. In this way Professor Hasselberg saved many lives.—*Jour. für Kind.—Brit. and For. Med. Rev., Jan.* 1852, *p.* 272.

46.—*A new Sign of Pleuritic Effusion.*—Dr. ROY, of Lyons, mentions, in the 'Revue Médicale,' the following sign of pleuritic effusion when the fluid has much diminished:—Put the left hand on the affected side of the chest, the patient siting up in bed; then percuss the ribs with the right hand. This will give rise to fluctuation, which will be easily perceived by the left hand. Dulness on percussion, absence of vesicular murmur, bronchial respiration, and ægophony, will of course remain the characteristic symptoms when the fluid is abundant.—*Lancet, Nov.* 29, 1851, *p.* 503.

DISEASES OF THE ORGANS OF DIGESTION.

47.—ON THE RELATION OF THE INCOME TO THE EXPENDITURE OF THE BODY.

By Dr. H. Bence Jones, F.R.S., &c.

[In the following article Dr. Jones considers the relation of the food to the body and to the excretions. He says:]

I have shown that the food, or income, consists of four classes of substances: that the flesh and blood contain the same classes—water, salts, non-nitrogenous organic matter, and nitrogenous organic substances. These four classes exist in all kinds of food and in all kinds of flesh; and flesh and blood I have taken as the representatives of the whole body.

We have seen that the elements in the food go into the body as highly complex compounds in a meroxidic state,—that is, not oxidized to the greatest extent,—while the elements in the excretions come out of the body in much less complex compounds—in a teleoxidic state—in consequence of the action of the inspired oxygen upon them. The substances passing out bear a direct proportion to the substances passing in. However different in mere appearance the principles going out and coming into the body may be, yet there must be a most intimate connexion between that which is going into the body and that which is passing out. I shall perhaps make this evident to you best by taking, as my illustration, a community rather than a single individual.

Let us suppose that a small, inhabited, fertile island is cut off from all external supplies of food, so that the income, that is, the produce, or the food, can only come from the island itself. The population increasing in that island, how can the food be made longest to suffice for the expenditure of the body—for the uses of the inhabitants? This is the problem. If the whole expenditure of the community—if each particle going out of their bodies was returned to the ground, and if the bodies themselves, after death, were returned to the dust, then every particle of the food would be restored to the soil from which it came. The elements might again and again go through the same course, or cycle; thus life might be prolonged, and the population might increase, and it would increase until some one or more elements for forming the food were wanting, and could not be procured. When some one element ceased to be present in the soil, the food would cease to be produced, and the increase of the population would stop; but if the whole expenditure, and the substances of which the bodies themselves consist were returned to the ground, the population might be kept up, as far as the substances which enter into the structures of the body are concerned, at the above-mentioned point for an indefinite time, and the population of the island might remain fixed and flourish at that amount, the expenditure and the income being exactly balanced. But if in such an island, instead of this care being taken of the substances passing out of the body, and of the constituents of the body themselves, if every means were taken to allow the expenditure to be lost, to drain it off, and carry it away from the island with the greatest care, the more rapidly it was drained off, the quicker would the island be exhausted, and be unable to furnish fresh income for the supply of the bodies of the inhabitants; exhaustion and starvation would follow as surely as bankruptcy and ruin result in the cases of thoughtless extravagance which we read of in the Bankruptcy Court. If a course of reckless waste and extravagant expenditure is entered on, there is but one way of avoiding the ultimate result,—that one way consists in putting as much into the pocket, or into the island, as is taken out of it. Hence the necessity of the importation of food, of guano, of bones, and mineral manures. The quantity that goes in, deducting always the increased weight of the population, gives the exact measure of the quantity that is thrown away; the income going in is on the surface, and is seen; the expenditure is in the deep, and is lost. The stream of corn and guano which arrives at our ports represents the under-current which is lost in the sea, always deducting the increased weight of the inhabitants of the island. Instead of thus leaving the elements of the corn and guano to nourish the sea-weeds, that support

the fishes, that feed the sea-birds, that produce the guano, how far more economical, how much surer and more rapid a return would be obtained by stopping the drain that passes into the sea. So in individuals: there must be a balance between the income and expenditure of the body; otherwise disease, increased growth, increased fulness, fatness, and plethora, or the opposite to these, must ensue.

If beefsteaks—that is, the muscles of an ox—are given to one who has taken strong exercise, and is in perfect health, this animal food, as I have shown, is dissolved, and passes into the blood, and is there used to supply the muscles of the man. The object for which food is eaten is not for the purpose of forming uric acid and urea, and the constituents of the urine. The muscle of the ox and the muscle of the man consist of the same four classes of substances. The muscle of the man, by exercise and by the action of oxygen, gives rise, as you saw, in all probability, to kreatin, kreatinine, uric acid, urea, and to the sulphates, phosphates, carbonic acid, and water. These substances are produced by the action of the oxygen upon the constituents of the muscle. The greater part of the waste passes off by the kidneys, the carbonic acid and water passing off by the lungs. These organs, the lungs and the kidneys, were not made to secrete urine and to throw off carbonic acid for the purpose of supplying vegetables; but they were made to separate useless or hurtful substances from the body.

The simplest view, then, which I take of any organ which is used, and must be repaired, is this:—The substance of the organ, by being used, becomes capable of being removed, and must be actually removed from the body. The waste of the organ or of the muscle passes off in the urine, while the food, or the income, is going on, and is used for the supply of the wasting body. Such I consider to be the very simplest view which can be given of the relation of the urine and the products of the respiration to the system; and, theoretically, it seems to be the only true healthy relation; and, perhaps, in a state of full bodily labour, when enough food, and no more, is taken, this may be the only relation existing between the organs of the body, or the muscles, and the excretions. But in the body provision has been made for too hard labour and for too much food. If too much food is constantly taken, and too little exercise, there must come an over fulness of the blood, and hemorrhage, unless some safety-valve for the excess had been provided. I have shown you in these lectures that the earthy phosphates, the sulphates, and the urates, are generally increased in the urine after food has been taken. If more food is eaten than is required for the wants of the system, there can be no doubt that the excess of food is thrown out by precisely the same organs that remove the waste of the muscles and the other structures. If even an excess of water is taken into the system, that excess is thrown out of the system without being used in the body at all, according to exosmotic laws which have not yet been clearly applied. We do not fully know the circumstances determining the removal of an excess of water, still less of other substances, from the body. As far as I can understand the question, it only adds to the difficulty to say that the unvitalised portion of the food or water is thrown out, and that the vitalised portion remains for the purposes of life. Why uric acid, for instance, is formed directly from the food seems a question more likely to be solved by keeping it distinct from the question of vitalization.

Long ago Dr. Prout fully recognised that food not only nourishes the body, but, when an excess is taken, passes out partly in the urine. That this double relation of the urine to the food and to the structures exists, I have attempted to show. The facts, then, though we cannot explain them fully, are,—that the food makes blood; that blood makes the muscle and tissues; that these, when used, return in a different form into the blood, and again pass out by the breath and the urine. But this, as I have said, is not the only relation of the food to the system; there is a shorter relation. Some portion of the food, if an excess is taken, and too little use of the system is made, goes into the blood, is not used for the purposes of the system at all, but passes out directly into the urine, when it nourishes vegetables, is raised again to a high state of complexity, and can again serve the purposes of animal life. This double relation, without doubt, exists; and though I cannot tell by what means the quantity thrown out, when an excess is taken, is determined, yet it is easy to see what is gained by preventing the system from being overloaded with nutriment; useless matters are thus removed and

saved from accumulating in the system, where they would rapidly produce illness, if not death. The first relation is that which I take to be the most healthy one; that in which the quantity of food is just proportionate to the wants of the system, and in which the substances thrown out are solely those which have been used in the system. The second relation I conceive to be only a relation of safety, not existing in the state of most perfect health.

As regards the expenditure, provided the actual quantity thrown out is proportionate to the quantity which is used in the system, it matters but little in what form the expenditure takes place. It makes no difference whether the expenditure of our money is in pounds, shillings, or pence, provided the same sum is spent; so, in this point of view, it is of little importance whether the substances pass out as uric acid, or urea, or carbonic acid, ammonia, and water, so that they are got rid of; the amount of the ultimate elements may be the same, notwithstanding very different substances may appear in the secretions from time to time.

One great cause of the variation in the forms of the substances passing out of the body is the difference in the action of the oxygen which is inspired, that is, whether the action be little or excessive. In the three lectures on respiration, I dwelt at such length on this action of oxygen on the food and tissues, that I have but little to bring before you here; but I may state this,—that, if the discovery of the circulation of the blood is still considered to exercise the greatest influence on physiology and pathology,—if the establishment of this fact is thought to have added more to our knowledge than any other single fact previously known in medical science,—if this circulation of the blood be considered as the one grand vital action,—I am sure that the grand chemical principle, the action of oxygen in the body, may be regarded as of equal importance. The action of the circulation and the action of oxygen cannot be compared; indeed, they admit of no separation: they are related, and there is even an intimate dependence on each other: they cannot even take place separately; the muscle could not contract without the chemical action, probably, of the oxygen; and the oxygen would not re-act on the muscle without the contractile action of other muscles assisting in drawing in the oxygen, and circulating it in the blood. There is no spot in the body to which this oxygen does not reach; in the capillaries, in the minute textures of various organs, the oxygen exercises its power of combining with hydrogen, with carbon, and even (as I have already shown you) with inorganic substances, oxidizing these to the greatest degree, oxidizing the hydrogen to form water, and the carbon to form carbonic acid. This action of oxygen it has been my great object to bring before you. Still, there are one or two other instances of the action of oxygen which I may be permitted to dwell upon here.

One, and a very beautiful one, as illustrating the action of oxygen on the excretions, is the peculiar colour which the urine possesses. Some of the various forms and hues of the deposits which it presents I have already shown you. Generally we might say, that yellow, brown, pink, and green, are the colours which, in every shade, and in every mixture, can be met with in the urine, in health, or in disease. If I were to ask where else you could find these colours in every variety mixed in nature, you would probably answer, in the changing leaves of autumn. The pale yellow of the poplar, the bright yellow of the ash, the brown of the oak, the pink of the climatis, the most varying shades of autumnal tints, can be matched by the urine in health and disease. But there is a much more wonderful relation than these mere variations of colour. The colouring matter of the bile, I have already mentioned to you, has been found to be identical with the chlorophyl, or colouring matter of leaves; and as the various modifications of the chlorophyl in autumn, by long exposure to the air, undergo various changes in colour, so, probably, that portion of the colouring matter of the bile which is absorbed undergoes various changes by the action of the inspired oxygen, and gives rise to the different colours which the urine possesses. Thus, even to the eye, the colouring of the urine bears evidence of that action of oxygen which is going on in the body. Perhaps the most beautiful and striking colour which the urine ever possesses, is the pink hue which you see here on this filter, on which is collected the sediment of the urine from a patient who has an inflammation in the neighbourhood of the liver, so that that organ is not able to perform its proper functions.

You might think, and it has been so stated, that this colouring matter bears a

relation to the colouring matter produced by the oxidation of uric acid; but I think a moment's experiment will serve to convince you that such is not the fact. It is, in my judgment, the result of a want of oxidation of the colouring matter of the bile rather than the result of any change in the uric acid. I have here a uric acid solution, coloured by purpurate of ammonia, as it was called by Dr. Prout, or murexid, as it is termed by Professor Liebig. Let me contrast this with the substance on the filter, which is not coloured with purpurate of ammonia, as I can show you. If I add to the solution of purpurate of ammonia a little potash, you will see that a most marked change ensues. Instead of remaining of a pink colour, it becomes of a beautiful purple—a very distinct and diagnostic re-action. This same colour would be produced if I added the potash to pure murexid, which I have here dissolved. If, however, I let the potash fall upon the sediment on the filter, the re-action will be very different,—we shall have a green colouring matter, precisely similar in appearance to that of the bile, and no purple colour at all. That this red colour is closely related to the colouring matter of the bile, I think probable, because, even in the bile itself, occasionally, a remarkable red colouring matter results from changes which take place within. Here is some bright red-coloured matter, consisting of altered bile, which I removed from the liver of a patient in St. George's Hospital. Thus even the very colouring matter of the urine may be an index of the oxidation which is taking place in the body.

Let me mention one more illustration of the action of oxygen, not only to show its action in health, but also to remind you of its purifying action in disease. I have already spoken of urea, which is one form in which the expenditure of the nitrogenous substances of the body is thrown off. I have here a solution of urea in alcohol; and if I burn the alcohol or oxidize it, what would happen? You know the result of the oxidation of alcohol alone—carbonic acid and water are formed.

Oxidation of Alcohol and Urea.

Alcohol + Oxygen = Carbonic acid + water

$$C_4\ H_6\ O_2 + O_{12} = C_4\ O_8 + H_6\ O_6$$

Urea + Oxygen = Carbonic acid + Water + Nitrous acid

$$C_2\ N\ H_4\ O_2 + O_{12} = C_2\ O_4 + H_4\ O_4 + N_2\ O_6$$

But, if I burn this alcohol that contains a little urea in solution, what would happen? Let me take a watch-glass, and pour into it a little of this solution of urea in alcohol. I will burn this, and show you the products of the oxidation of the urea as well as the alcohol. I shall find, if the combustion takes place as I wish, that the urea combines with the oxygen, and that we have not only carbonic acid and water produced, but an oxide of nitrogen—nitrous acid resulting from a combination of the oxygen with the urea. You remember my test for nitrous acid. I have here some starch, iodide of potassium, and a little dilute hydrochloric acid, with which I moisten the sides of this glass, and hold it over the solution which is burning in the watch-glass. See how rapidly there is manifest evidence of nitrous acid being formed by the beautiful blue iodide of starch that is produced. If I take this urea into the body by swallowing it, it is oxidized in the same way as it is out of the body. You will remember what I stated regarding the salts of ammonia. Urine passed after muriate of ammonia was taken, I showed you, contained manifest traces of nitrous acid: so, also, precisely the same thing can be proved to occur when forty grains of urea are taken, the urea is oxidized in the body just as it is when burnt out of the body. If urea or muriate of ammonia or other substances accumulate in the body, they will probably be oxidized there, as I have shown; even alcohol, when taken in large quantities, is a poison, and its removal from the body depends upon the intense action of the oxygen which is inspired.

In the healthy state, then, the four classes of substances of which our tissues and bodies are composed pass out of the body thus: 1st, the water passes out in respiration, by the perspiration, and by the urine; it passes out as vapour and as liquid. 2ndly, the salts, or the mineral matters, oxidized, as I mentioned, to the greatest degree, pass out also chiefly by the urine. 3rdly, the non-nitrogenous organic substances of which I have spoken, including the fatty matter which circulates through the system, also are oxidized in the body, and pass out in health in

the form either of carbonic acid and water only, or pass out as acetates, lactates, or even possibly as oxalates, in the urine. In states of disease these non-nitrogenous organic substances pass out of the system as sugar. 4thly, the nitrogenous organic substances are expended in the urine as uric acid, kreatin, urea, or in another form of urea, namely, carbonate of ammonia. In disease, the albumen itself, as I showed you, is occasionally drained away.

The specific gravity and quantity, in twenty-four hours, of the urine thrown out of the body may thus be taken to represent, for the most part, three out of the four grand classes of substances excreted from the body: these are, 1st, water; 2ndly, the salts; and 3rdly, the nitrogenous organic substances; while the non-nitrogenous organic matter is, for the most part, thrown out by the lungs. As regards three of these classes, then, the quantity of the urine may be taken to represent the expenditure of the body, provided the amount of the substances held in solution in that water is determined. The specific gravity of the urine does not accurately represent the quantity of the substances passing out in solution; the specific gravity cannot be taken as a true and perfect index of the quantity of substances that are held in solution. If I take equal quantities of the same substances dissolved in precisely the same quantities of water, I shall find the specific gravity of the solutions may be very different.

The following table shows, that in 1000 grammes of water at 60°, 40·3 grammes of urea gave 1010·4 specific gravity; but the same quantity of common salt gave 1024·1. The same quantity of sulphate of potash gave 1029·7.

Relation of Specific Gravity to Solid Residue.

In 1000 grammes of Water at 60°		Specific gravity.
40·3	grammes Urea	gave 1010·4
20·1	"	" 1005·3
10·0	"	" 1001·6
40·3	grammes Common Salt	" 1024·1
20·0	"	" 1011·9
10·0	"	" 1006·0
40·3	grammes Sulphate Potash	" 1029·7
20·0	"	" 1014·0
10·0	"	" 1007·1

So 20 grammes of each substance dissolved in the same quantity of pure distilled water, gave different results as to their specific gravity; this being, as you see, in the instance of urea 1005·3, and in the instance of salt 1011·9, and of sulphate of potash 1014. By taking only 10 grammes similar results were obtained, and so with intermediate numbers also. I find, also, that if you take the urine at different times of the day, and examine its specific gravity, you may find that it has precisely the same specific gravity, and yet the quantity of solid substances dissolved in it may be very different. This is shown in the following table:—

Relation of Specific Gravity to Solid Residue per 1000 *grains of Urine.*

		Specific gravity.	Solid residue.
Urine passed	before dinner	1028	67·0 grains per 1000.
"	after "	1028	66·6 "
"	before "	1028	64·8 "
"	after "	1034	84·6 "
"	before "	1025	60·8 "
"	after "	1025	64·6 "
"	before "	1025	56·7 "

The different quantities of solid matter in the same quantity of urine of the same specific gravity are explained by there being different quantities of urea or of salts in solution. The salts add more to the specific gravity than the urea does.

The way of determining the quantity of solid matter is to take a weighed quantity of urine, and evaporate it in a water-bath to prevent the destruction of the urea, which is a substance easily decomposed. Before it comes to perfect dryness it is put into the vacuum of an air-pump over sulphuric acid, which takes away the residue of water, and it must be weighed until no further loss ensues. If you *evaporate over the* water-bath alone you can never bring it to perfect dryness; it is

always necessary to place it *in vacuo* over sulphuric acid. The specific gravity alone may give you a rough conjecture as to the amount of solid matter in solution; and there are various methods by which the specific gravity can be ascertained. These graduated instruments may be used for this purpose; or, still better, for small quantities, these little glass beads can be used. The specific gravity of the solutions is determined by seeing whether the beads will sink or float. Numbers are marked on the different beads, so that the specific gravity of a very small quantity of urine can be ascertained, but far more accurately by the 1000 grain bottle and a good balance. You will find tables given in books which profess to tell you the solid residue of urine of all specific gravities. However, it is impossible to arrive at the amount of solid substances by any of the tables which have been published for this purpose. Careful evaporation alone, in every case, can give you accurate results.

If we could accurately determine the quantity thrown out by the lungs, skin, and kidneys; and if, at the same time, we could determine the quantity of food which is taken in, we should arrive at the fact of the gain or loss of the body. This knowledge is obtained far easier and more simply by weighing the body itself. We thus arrive at the loss or gain much more readily than by deducting the expenditure from the income. When the fact of a loss or a gain is determined, there are two possible ways to account for it. 1st, if the body lose, too little food has been taken in or too much has been given out. In starvation life may be prolonged either by giving food or by taking the least possible exercise,—by causing the least possible quantity of substance to be removed from the body. In diabetes, if you stop what is passing out, to a certain extent you check the progress of the disease; and the same result is obtained by giving an excess of oleaginous food. 2ndly, if the body is gaining instead of losing, too much must come in, or too little must go out. In excessive fulness, or in gout, less food must be eaten, or more exercise must be taken, or, by active medicines, the excess which has accumulated must be removed. Thus, here as elsewhere, Nature has more than one way by which her object can be attained. She must be assisted in her work, not so much by compulsion and violence, as by watching the indications she presents,—that is, by following the suggestions she offers. Thus the physician can be the means of restoring that balance between the income and the expenditure, without which we cannot long go on.

If in these lectures I have been enabled to show you the value of animal chemistry,—if I have convinced you of the importance of the knowledge already obtained, and of the far greater value of that which has to be acquired,—if I have satisfied you that chemical actions are taking place in the body, and more especially, that the action of oxygen never ceases therein,—and if I should thus lead you to see that the vital force is no separate, distinct, and individual force, but that it is a collective term including many distinct, though, very probably, closely related forces —as, for instance, the nervous force, the contractile force, the chemical forces, and the formative forces,—I shall not, I hope, have occupied your time and attention (for which I am indebted to you) in vain.

Life.—The Conjoint Action of many Separate Forces in the Body.

Mental forces			Animal forces.
Sensation			
Contraction			
Cell forming		Vegetable forces.	
Cell modifying			
Chemical forces	Inorganic forces.		
Gravitation			

Finally, I have as far as possible endeavoured to base these lectures on experiment, because in chemistry no other authority than that of experiment can with safety for one moment be trusted. In animal chemistry more perfect and more extended experiments are wanted to enable us to arrive at clearer and more connected ideas; and let us never forget, that no medical opinion, no chemical authority, ought to be allowed the slightest weight in opposition to experimental truth.—*Med. Times and Gazette, March* 6, 1852, *p.* 231.

48.—CASE OF INFLAMMATION AND ULCERATION OF THE THROAT AND TONGUE,

SUCCESSFULLY TREATED BY NITRATE OF COPPER.

By Dr. William Moore, A.B., Trinity College, Dublin.

[In this case the uvula and soft palate, with the structures posterior to these, had been removed by the ulcerative process, extending deeply also at the base of the tongue, partially destroying the epiglottis. The mucous membrane presented an ulcerated and abraded appearance. A trace of tonsil remained; but from pain affecting the right ear, it is natural to suppose the ulcerative process had extended along the Eustachian tube. On examining the heart, a most abnormal sound was heard, caused, it was supposed, by scrofulous excrescences on the valves of the heart, more particularly on the aorta. The following treatment was adopted:]

℞. Pil. ferri iodidi x; one to be taken twice daily. ℞. Pil. aloes cum myrrhâ x; two to be taken every third night. The tongue and throat were ordered to be touched with caustic iodide, the formula of which I subjoin. ℞. Iodinii, et iodidi potassii, singulorum 3iv; aquæ destillatæ, ℥j. M. Fiat solutio.

Dec. 23. The patient presented herself this morning; and, to our no small amazement, the "whooing sound" had wholly disappeared, both sounds of the heart being distinctly audible and normal. The pulse was 98, regular but weak. There was no marked improvement in the throat or tongue; the latter still appeared fissured, hard, and knobby. A copious leucorrhœa had supervened.

Dec. 30. The throat was cleaner, and presented a more healthy character; the ulcerative process was somewhat checked. The tongue was still covered with small white patchy ulcerations. The patient could swallow with much less difficulty. The heart's action was natural. The leucorrhœa still continued. She was ordered to continue the aforementioned treatment, and to use the *gargarisma aluminis*, in addition to the local application of the caustic iodide.

Matters continued gradually improving under this treatment till early in March, at which date, being obliged to leave this locality, I lost sight of the case till the end of the following May. On the 2d of last June, the patient again presented herself at the dispensary; and, on a careful examination of the throat and tongue, I found manifest symptoms of retrogression. She stated that latterly she had been very remiss in her attendance at the dispensary, and consequently felt much worse. The sloughy discharge again adhered to the hard palate; the tongue was ulcerated in parts; and the patient complained of general debility. The heart's action was feeble, both sounds were distinct; and the cough was harsh and very troublesome. Sensible of the good effects of the previous treatment, whilst properly attended to, I determined again to have recourse to somewhat similar remedies, and accordingly prescribed mistura ferri composita with tincture of iodide, and a gargle of dilute hydrochloric acid and tincture of catechu to be used at the same time. As so many caustic applications had been applied, and to a certain extent ineffectually, I touched the throat and tongue with *nitrate of copper;* a caustic, as yet, comparatively little used, but to the incalculable benefit derived from the application of which, not only in this but also in many similar cases, I can bear the fullest testimony. After this treatment had been persevered in for about a fortnight, the throat had become quite clean, the sloughy discharge had almost disappeared, the tongue was also clean and free from ulceration, and the general health was much improved. Being now satisfied of the good effects arising from the first application of the nitrate of copper, I determined to repeat it, continuing the gargle as before; and I prescribed at the same time a dessert-spoonful of cod-liver oil to be taken three times a day.

[From this time, under the same treatment, the case gradually improved, though the voice was lost from the early destruction of the vocal apparatus.]

The results demonstrate the value of nitrate of copper as a caustic; a remedy which I have also found effectual in similar cases, where, in spite of the ordinary topical treatment, the disease had remained unchecked. Its caustic properties may be objected to as being too powerful, and its application may be alleged to be fraught with danger, but in every case in which I have used it, no bad effects have ensued, nor is there reason to apprehend any if the following precautions be

attended to; viz., to dry the ulcer or part to be cauterised before applying the nitrate, and afterwards to smear it with oil. For further information relative to the successful application of this escharotic, the reader may consult an excellent treatise on 'Diseases of the Tongue,' by Dr. Fleming, in No. xix. of the 'Dublin Quarterly Journal of Med. Science,' p. 87.—*London Journal of Medicine, Jan. 1852, p. 32.*

49.—ON CHRONIC COLICA PICTONUM.

By Dr. William Norris, Stourbridge.

[Dr. Norris believes that a chronic form of this disease is by much the most frequent, and that often the health suffers with irreparable injury to the constitution, because precautionary measures have not been adopted early. In two severe cases, the patient suffering from great pain for many weeks, Dr. Norris confesses he was not aware of the nature of the disease until he had examined the gums; hence the necessity of examining the occupation of those who apply to us with disease of the digestive organs:]

I will endeavour to give a concise account of this disease, which may run on for years without assuming the acute character; it may be denominated chronic painter's colic, and in my humble judgment ought to be distinctly classed by all systematic writers on medicine.

When persons have been employed several years, and sometimes sooner, the gums become enlarged and flabby, not blue; they have often pains in the loins, or some part of the muscular system, and soon look sallow and thin, with more or less disorder in the digestive organs, and sometimes a sweet taste in the throat; these symptoms may go on ten or twenty years before they end in the acute or chronic form of the disease, when the leaden tinge in the gums will be found, proving the system to be now saturated with lead; and if the disease assume a chronic character, the symptoms will be somewhat similar to the acute, in a milder degree —namely, constipation of the bowels, sometimes diarrhœa; a coppery taste; tongue covered with a dark cream-coloured mucus; a griping pain in the bowels many hours during the day and night, often around the umbilicus, extending over many parts of the abdominal region; loss of appetite, nausea, and sometimes vomiting; often an exceedingly sweet taste in the throat and fauces; great depression in the nervous and muscular system; seldom excitement in the vascular system; pulse generally slow and feeble; the skin dry; and all the secretions diminished; kidneys torpid; urine scanty, and of a dark colour.

Treatment.—Mild purgatives daily, or every other day; calomel and opium every six or eight hours; then iodide of potass in one or two grain doses. If the pain is frequent and distressing, occasional doses of laudanum. Diarrhœa is sometimes kept up by scybala or vitiated secretions, and may require castor-oil previous to the use of astringents. The diet should be light and nutritious; dilute freely with mucilaginous drinks. When pain is usually severe, the warm-bath has a striking effect; bran poultices and hot salt are always usefnl, and may be applied to any of the affected parts.

When patients have premonitory symptoms, they should be removed from their employment, the skin frequently washed, and the clothing changed, with proper remedies; by these precautions, many serious attacks may probably be prevented. The sweet taste from the throat may be removed by frequent gargling with water, or water mixed with iodide of potass.

Those who are predisposed to the disease may take iodide of potass once or twice every year; "and as a preventive take sulphuric acid lemonade." The patients should always wear warm clothing.

Chloroform has lately been used by the French. The usual practice of Dr. Aran is, to apply a compress dipped in chloroform to the abdomen for the space of twenty minutes, and at the same time to give internally about thirty drops of mucilage. These doses are repeated according to circumstances. He has treated eight cases in this manner, all of which recovered in from two to six days, (see 'Provincial Journal' for March, 1851.) I think chloroform may probably be used with

advantage, and may diminish pain in the acute species, and be useful in the chronic form, and I have no doubt many of the suffering patients will willingly submit to its use in any way.—*Lancet, Jan.* 3, 1852, *p.* 6.

50.—TÆNIA SOLIUM—RELAPSE AFTER THE USE OF THE KOUSSO.

By JAMES VAUGHAN, Esq., Assistant Surgeon, Bombay Army.

[Mr. Vaughan says the success of the kousso depends very much upon the quality used, as he has observed many specimens differing in many important particulars.]

But there is another cause which has an undoubted influence in affecting the efficacy of the kousso,—viz., *the quantity administered;* and some defect in this respect may have led to failure in the three cases treated by Dr. Wilson. In two of these "the usual dose" was given, which, I presume, was four drachms. In case No. 2, six drachms were prescribed. My own experience of the drug leads me to conclude that, as with other medicines, the quantity of kousso to be administered for the expulsion of the tænia must depend upon the idiosyncrasy of the patient. A case occurred under my own inspection, of an officer who took the usual dose two or three times, and the result was much the same as in the individuals treated by Dr. Wilson. He then took an ounce and a half, since which several months have gone by, and he has not been troubled with any symptoms of a relapse. But in addition to the reason just adduced for increasing the dose of kousso in particular cases, there is another which deserves to be noticed. The drug is brought from Abyssinia, sewn up in small skins, and when opened for examination, the contents are usually found mixed with several heterogeneous substances, such as straw, stalks and leaves of other plants, sand, &c., the result of carelessness in the gatherers, or, what is more probable, of knavery in the sellers. To what an extent this negligence or fraud is practised may be judged from a late instance which fell under my own observation. A parcel of kousso, which in the skins weighed ninety pounds, after it was properly picked and cleaned, weighed only fifty pounds.

I have found the following the most effectual mode of administering the remedy. The drug is left to macerate in hot (but not boiling) water for three hours; it must then be drunk by the patient *entire*, without being strained, after a previous fast of several hours. When thus given, I can say, with the writer of the article in the 'Lancet,' no patient has been known to apply for further treatment. Of course it is to be understood that a sufficient dose is administered.—*Lancet, Jan.* 10, *p.* 40.

DR. HANNON, of Brussels, has used the kousso in the form of injection, and by the mouth, followed by manna, with great effect in children tormented with ascarides. Sometimes, after all other means (aloes, senna, &c.), had failed, two or three enemata effected a complete cure. Hanna considers it to be the best vermicide known.—*La Presse Méd.—Med. Times and Gazette, April* 10, 1852, *p.* 375.

51.—*Cases of Tænia treated by Male-fern Oil.* By ROBERT MOLLOY, Esq., and W. M. F. CHATTERLEY, Esq.—[In Mr. Molloy's patient the symptoms had for the last three or four months become so distressing and annoying that he was at times unable to continue his employment.]

On the 5th ult., I directed him to take, on retiring to bed, a powder composed of four grains of calomel and two of ipecacuanha, together with a strong draught of concentrated compound aloes decoction; at six o'clock on the following morning, he had a drachm of fern-oil, obtained from Morson's. By the action of the draught and powder, a copious evacuation took place at four, a.m., bringing away a large quantity of joints and other debris of the worm, and at twelve at noon, just six hours after taking the oil, the whole of the worm was expelled. It measured more than two yards in length, was very perfect, and had twisted itself *into two knots; the* first eight inches below the head was remarkably complicated,

and cost me some time and patience to unravel; the second eighteen inches lower down presented nothing peculiar.

[Mr. CHATTERLEY relates the following case:]

J. B——, living at Walbrook, aged eighteen, had been long troubled with tape-worm, for which I desired him to fast from the evening of the 4th Jan., 1851, to the evening of the 5th. This was done (so at least he said, and his family confirmed), with the exception of some weak tea, and a small cup of thin gruel. On that evening he took six drachms of castor-oil, and on the following morning, the whole of this mixture: tincture of male-fern, one drachm and a half; compound tragacanth powder, half a drachm; water, three ounces,—to be followed by at least half a pint of weak tea. A little thin gruel was allowed during the day; and in the afternoon, six drachms of castor-oil again. In the evening, at one effort, the whole of a tape-worm, measuring twenty-two feet in length, was expelled; and he has since been free from so unpleasant a companion up to the present time, (more than fourteen months).

A little girl, aged five years, who was frequently in the habit of passing a yard or two of tape-worm, at a time, had half a drachm of the same tincture (Hooper's) given in pills of extract of liquorice, which she vomited almost immediately, and which produced no effect on the worm. She had previously fasted about eighteen hours, at my house, under my own inspection. I may add that kousso had been administered about four months previously, at King's College Hospital, when many yards had been expelled, but, I believe, not the head; and if this do not take place, as is well known, the entozoa will continue to multiply.—*Lancet, March* 6, and *April* 3, 1852, *pp.* 237, 329.

52.—ON SULPHURIC ACID AS A CURE FOR DIARRHŒA.

By DR. HENRY WILLIAM FULLER, Assistant Physician to St. George's Hospital, &c., and T. BUXTON, Esq., Westminster.

[Mr. Griffiths, in his original letter in the Lancet, about three months ago, was the first to notice the marvellous efficacy of sulphuric acid in diarrhœa; so marvellous indeed that Dr. Fuller believes it is destined to effect a complete revolution in our treatment of this troublesome, and not unfrequently, dangerous class of maladies. The following cases are given as examples of its effects:]

Thomas Goodwill, aged 49, a gentleman's servant, was admitted into the Fuller ward of St. George's Hospital, on the 6th of September, 1851. He had been quite well the previous night; his bowels had acted comfortably; and he had eaten nothing likely to disagree. Such was his history up to 7 a.m., on the morning of his admission, when he was seized with severe pains in the bowels, cramps in the extremities, vomiting, and pale, watery purging.

At 11 a.m., when he applied for admission into the hospital, these symptoms continued with increased severity; he was doubled up with pain in the belly, and his extremities were drawn into knots by cramp. Indeed, so excruciating were his sufferings, that he had to be carried up to bed. His countenance was anxious, his features were pinched and drawn, his extremities icy cold. His tongue was very dirty, and dry in the centre, but moist at the edges; his pulse rapid, small, and very weak; his urine scanty, pale, or indeed almost colourless. He was ordered, as a draught to be taken every hour—

Acidi sulphurici diluti ℥ xx.; aquæ ℥ iss.

He vomited about a quarter of an hour after taking the first draught; but, after taking the second, which he did immediately on the rejection of the first, he neither vomited, nor was purged, and when I visited him at 1 p.m., he was quite warm, and comparatively free from pain. I therefore ordered the medicine to be repeated every second hour only.

At 4 p.m., I found him warm and perspiring, and almost free from pain. He had been sleeping quietly for about an hour and a half; his bowels had not acted again, nor had there been the slightest return of nausea or vomiting. The medicine was, therefore, repeated at still longer intervals, (every three hours,) and was omitted altogether for a time, during the action of a rhubarb draught which I ordered him to take in the evening.

On the 7th, (the following day,) I found that he had passed a comfortable night, and had slept well. There had been no return of the pain, nausea, vomiting, or purging. His bowels had acted once after the rhubarb, and his tongue was much cleaner.

Rep. haustus acidus 6to quâque horâ. Haustus rhei cras mane.

On the 8th, he was quite well; but I kept him in the hospital till the 9th, lest there should be any recurrence of the symptoms. Such, however, was not the case: and he was therefore discharged on the morning of the 9th.

The other case is of the same character.

Charles Hanscomb, aged 64, was admitted into Fuller ward, at 1 p.m., on the 9th of September, 1851. His attack, which had not been preceded by any disorder of the bowels, nor by any irregularity or imprudence of diet, commenced suddenly on the night of the 7th, with vomiting, purging, and crampy pains in the abdomen. During the thirty-six hours which elapsed between his seizure and his admission into the hospital he had no medical advice, and the symptoms, which at first were comparatively slight, increased gradually in severity, and became accompanied by violent cramps in the extremities.

On admission into the hospital, his features were collapsed; his face was of a bluish tinge; his expression anxious. His extremities were icy cold, and had lost all power of sensation; his breath also was cold, and he was shivering violently. Indeed, he stated that he had not ceased shivering for the last six hours. His tongue was yellow and furred, and rather dry; pulse 100, extremely weak. He was vomiting and purging incessantly, and, indeed, vomited once and was purged once during the short time which elapsed before he could be got into bed. The dejections were pale and watery, and the matters rejected from the stomach acid and almost colourless.

I ordered: Acidi sulph. diluti ℥ xxv., aquæ ʒ iss. M. ft. haustus statim sumendus et omni horâ repetendus ad tres vices.

At half-past three p.m., I found him warm and perspiring, and comparatively free from pain. He had vomited about twenty minutes after taking the first dose of the medicine, but a second dose was then immediately administered; and since that time there had been no return of nausea or purging, and the cramps had entirely ceased.

Rep. haustus acidus 2dis horis tantum.

Six p.m.—No return of vomiting or purging, and has no longer any pain in the abdomen or in the extremities.

Rep. haustus acidus 6tis horis si opus sit.

Sept. 10.—Slept soundly all night; omitted the medicine after the first five doses, and has had no return of pain, sickness, or purging. Feels quite well, and is going to leave the hospital this morning, the tongue being moist and almost clean, and the bowels having acted once comfortably.

I have thought it right to give the above details, as entered in my case book, without comment; but I would add, in explanation, that I have seldom seen more alarming cases. In both instances the collapse was very great; in both the extremities were quite cold, and the cramps in the abdomen and extremities were very severe: in both the tongue was exceedingly furred, the urine was nearly suppressed, and the alvine evacuations were pale and watery. No cases can be conceived less likely, *à priori*, to be benefited by the administration of sulphuric acid; yet in both the happiest results ensued. No vomiting nor purging occurred in either instance after the second dose of the medicine, or in other words, after the treatment had been continued half an hour; and at the expiration of an hour and a half, when three doses had been taken, the patients were warm and comparatively well.

In the first seven doses of the medicine, and in the second five doses only were given; and in the first a rhubarb draught was administered only because my preconceived ideas had led me to imagine what subsequent experience has proved not to be the fact, viz., that the administration of some purgative is necessary to carry off offending matters from the bowels.

Since the cases just detailed fell under my care, I have had ample opportunities of testing the value of sulphuric acid in diarrhœa, and I can speak confidently as to its curative power in such cases. I have administered it in private practice, and in my practice at St. George's Hospital, to twenty-seven patients, the whole of whom, with three exceptions, derived immediate and striking benefit. The sickness *speedily abated*, the purging ceased, and the pain in the abdomen shortly sub-

sided. In the three instances in which it failed to produce its ordinary curative effect, there had been considerable disorder of the stomach and bowels for many days before the attack. Two of the patients had been exceedingly costive and bilious for above a week, and had previously suffered from occasional relaxation of the bowels, with dark coloured and offensive motions; and the other patient had been suffering from alternate constipation and relaxation of the bowels, for a period of more than three weeks, and had taken large quantities of chalk mixture.

In a practical point, the value of this remedy is greatly increased by its being so cheap, so extremely palatable, and suitable to all ages. By charitable institutions for the relief of the sick poor, it can be largely employed at very little cost, and by the youngest persons it will be taken, when mixed with syrup, without the slightest repugnance. In five minim doses, I have given the dilute acid to a child of sixteen months old with perfect success; and I have known it given with the happiest results to children of a still more tender age.

The only question of doubt or difficulty is as to the class of cases in which it is most serviceable, and whether it is positively injurious in any. I have already pointed out the nature of the cases in which it has failed in affording relief; and I may add, that its effects have appeared to me remarkable, in proportion to the suddenness and severity of the attack. Severe attacks, which commence suddenly during the prevalence of an epidemic influence, without any previous derangement of the stomach, and which are accompanied by depression and by severe crampy pains in the abdomen and in the extremities, are certainly checked far more rapidly by the acid than by any other treatment, however active. But an attack of ordinary bilious vomiting and purging, which has been preceded by derangement of the primæ viæ, is relieved, I believe, by calomel and opium, followed by a rhubarb draught, and by some warm antacid and astringent medicine, as quietly and effectually as by sulphuric acid. Indeed, in such cases, I have been induced to give a decided preference to the older and more complex method of treatment.

The question whether the acid may not sometimes prove injurious, can only be answered satisfactorily when we have more extensive data for an opinion: and I trust that all gentlemen who have had any experience of the effects of the acid in these cases, will make known the results of their observation. My own experience does not lead me to anticipate any mischievous result from its cautious exhibition, nor among those of my friends who have administered it largely can I hear of an instance in which it has proved injurious.

[In the form of a letter to the Editor of The Medical Times and Gazette, Mr. Buxton observes, with regard to the use which he made of sulphuric acid in the treatment of cholera:]

At first, when I treated such diarrhœa with sulphuric acid, I myself was a little disappointed in its efficacy; it never failed to relieve immediately, but was not so magical in staying at once the disease; but I soon perceived that I had not administered it in sufficient doses, for a patient being attacked with diarrhœa in the middle of winter, after some days sent for me. I prescribed two drachms of the diluted sulphuric acid in six ounces of water; an ounce to be taken directly, and repeated after each motion, or when there was any pain. At my visit the next day, my patient told me the first dose relieved him, but he soon had a slight return of the pain, and that he had taken one mixture at repeated doses the previous evening. He had slept well, but this morning while dressing he felt slight pains in the bowels; he had sent for another mixture and took a wineglassful. Breakfast causing a slight return, he had taken another wineglassful, and previous to leaving home, fearful he might be purged, he had taken another wineglassful, and finding not more than a table-spoonful remaining in the bottle, he took it also. This was two drachms of diluted sulphuric acid in two hours for a tedious but not acute diarrhœa. My practice had always been to administer twenty to thirty minims at repeated intervals, as the case dictated; but this case emboldened me, and I now, in ordinary cases of diarrhœa, give much larger doses than in epidemic cholera. I have never yet found it fail, nor have I experienced any bad effects from its administration in large doses.

The advantages of sulphuric acid over the usual treatment of calomel, opium, &c., are: the disease is more rapidly checked, the sleep is pleasant and refreshing, the tongue is clean, and the appetite quickly returns, and, except in a few cases, it is not

followed by thirst or febrile symptoms. With calomel and opium, the disease is more tedious in duration, the tongue is foul, the head aches, there is great debility, little or no appetite, with a hot, dry skin, a tedious thirst, and the patient requires some other medicine to alleviate these symptoms, which the remedies of calomel and opium have produced.—*Med. Times and Gazette, Jan.* 10 *and* 24, 1852, *pp.* 32 *and* 92.

53.—*On the Tartrate of Soda as a Purgative.* By M. Delioux.—M. Delioux recommends the crystallized tartrate of soda as a most agreeable and certain purgative, being quite equal in power to the sulphate of soda or magnesia, and not repugnant to the taste. The medium dose, for active purgation, is ten drachms, little or no colic attending its action. The sulphate, phosphate, and tartrate of soda, and the tartrate of soda and potassa, may indeed be substituted for each other as regards their purgative action; but the tartrate of soda surpasses them all in pleasantness of taste. M. Delioux is no believer in the doctrine which connects the purgative action of the body with its sapidity, the purgation resulting from the indigestibility of the body swallowed, and the exosmosis it gives rise to. —*Bull. de Thérap.—Brit. and For. Med.-Chir. Review, Jan.*, 1852, *p.* 277.

54.—*On the Cause of Abscess of the Liver.* By Dr. Budd.—Admitting dysentery or ulceration of the bowel to be a source of abscess of the liver, it is obvious that the liver does not become involved by spreading of the inflammation, but by some contamination of the portal blood. This may be either by *pus* formed by suppurative inflammation of one of the small intestinal veins; or by matter of other kind resulting from softening of the tissues; or by the fœtid, gaseous, and liquid contents of the large intestine in dysentery. It seems probable that contamination of the first kind usually gives rise to small, scattered abscesses; of the last, to diffuse inflammation, and a larger, perhaps single, collection of pus. If the morbid matter be such that it does not mix readily with the blood—as globules of pus or mercury—it will cause small, circumscribed abscesses, the rest of the liver being healthy. If, on the contrary, the morbid matter be readily diffusible in the blood, all the blood will be vitiated, and diffuse inflammation result.—*Lancet, Dec.* 13, 1851, *p.* 556.

DISEASES OF THE URINARY ORGANS.

55.—ON THE PATHOLOGY AND DIAGNOSIS OF RENAL DISEASES.

By Dr. George Johnson, Assistant Physician to King's College Hospital, &c.

[The first disease Dr. Johnson mentions is that of acute desquamative nephritis, characterised by a desquamation of epithelium from the uriniferous tubes. It is very frequently met with, and is generally associated with cases of inflammatory dropsy. The following observations have chiefly reference to the condition of the urine and of the kidneys:]

In the early stages of the disease the urine is usually scanty, high-coloured, and more or less deeply tinged with blood. It is highly albuminous, becoming almost solid on the application of heat or nitric acid. The specific gravity varies, being quite as frequently above as below the normal standard. The urine deposits a copious dark-brown sediment, which, on a microscopical examination, is found to contain numerous casts of the uriniferous tubes, composed of fibrin, in which are entangled entire cells of renal epithelium and blood corpuscles; numerous cells of the same kind, also blood-corpuscles and irregular masses of coagulated fibrin, are scattered over the field of the microscope. It sometimes happens that the patient dies at this early period of the disease, the immediate cause of death being an *attack of convulsions*, followed perhaps by coma; or it may be that one or more

of the serous membranes or the lungs have become inflamed, the blood being poisoned with the urine which the kidneys have failed to excrete.

When in such circumstances an opportunity offers for examining the kidneys, they are found to be slightly increased in size, and more considerably in weight; the latter, in consequence of the increased density resulting from the accumulation of morbid products.

The cortical substance presents the mingled appearances of anæmia and vascular engorgement; small hemorrhagic spots are frequently scattered over the surface. The medullary cones are usually congested, and have the dark hue of venous blood; not unfrequently the mucous membrane of the pelvis presents some appearances of increased vascular engorgement.

On a microscopical examination, most of the convoluted tubes are found to be opaque from containing excessive numbers of epithelial cells, which, by a process of desquamation, have been thrown off into the cavity of the tube. Others of the tubes are filled with blood, and occasionally the Malpighian capsules at the extremities of these tubes may be seen to contain blood which has evidently escaped from the ruptured Malpighian capillaries. The tubes filled with blood constitute the hemorrhagic spots before mentioned, which were supposed to be enlarged Malpighian bodies until Mr. Bowman demonstrated their true nature.

The Malpighian capillaries in this and in every form of acute renal disease, attended by an effusion of serum from these vessels, are somewhat opaque, so that the blood corpuscles within them appear colourless, the surface of the vessel is sometimes roughened, and their outline is rendered distinct apparently by the effusion of coagulable materials in their interspaces. It rarely if ever happens that there is any appearance of an organised effusion in the Malpighian bodies. Neither the arteries nor the intertubular capillaries present any change of structure.

The pathological explanation of the condition of kidney which I have described appears to be this,—that the blood contains some morbid or abnormal material, *e. g.*, the poison of scarlatina, which may have been driven in from the cutaneous surface by exposure to cold. An effort is then made to eliminate this poison by the renal epithelium, which is cast off by a process of desquamation analogous to that which the same poison naturally excites upon the surface of the skin. It is probable that until the cutaneous desquamation has ceased, the patient may impart the disease to others, for the continuance of this process is an indication that the poison is still passing off in combination with the epidermic scales. In like manner it is probable that some of the poison is conveyed away by each cell which is shed from the uriniferous tubes of the kidney. I have already alluded to the fact, that in the normal state no renal epithelium is visible in the urine, and that the appearance of epithelial cells is an evidence and a consequence of disease. In many instances it is impossible to ascertain the nature of the abnormal product which excites this renal desquamation, but in one case, at least, it is not difficult to determine this point. When from any cause the functions of the liver are imperfectly discharged, so that the bile is permitted to accumulate in the blood, the kidneys make an effort to eliminate some of the biliary constituents, and on a microscopical examination, the urine is found to contain cells of renal epithelium deeply tinged with the new materials. It appears, therefore, that in the effort to eliminate bile, some of the renal epithelium becomes detached, and so is washed away with the secretion.

From a consideration of these facts, viz., that in the natural and healthy condition, no renal epithelium appears in the urine; that during the progress of scarlatina the desquamation which seems to be the natural means of eliminating the poison from the skin may be transferred to the kidney; that a similar process of desquamation results from the effort to eliminate bile by the kidneys; and, further, that this renal desquamation never occurs except under circumstances, such as I have already referred to, which are calculated to produce a morbid condition of the blood;—from a consideration of all these facts, it appears reasonable to conclude that the appearance of renal epithelium in the urine affords presumptive evidence, first, that the composition of the blood is abnormal; and, secondly, that some of its abnormal constituents are being eliminated by the secreting cells of the kidney. The process of renal desquamation, which is primarily and essentially wholesome and beneficial, may yet be productive of some secondary, and as we may say acci-

dental ill consequences; for the secreting surfaces in the kidney being arranged in the form of minute tubes of great length and very tortuous, it is obvious that the desquamated epithelial cells cannot freely escape, like the epidermic scales, from the cutaneous surface, but that they must frequently fill, distend, and obstruct the tubes, and thus greatly impede, and sometimes entirely arrest, the secretory process. And thus it happens, that many tubes, being rendered inefficient either for the further elimination of the morbid products or for the discharge of their normal excretory functions, the urine is greatly diminished in quantity, and the patient may die from some of the secondary consequences of an accumulation of poisonous excrement in the blood.

With reference to the origin of renal disease in an assumed morbid condition of the blood, it is important to observe the fact, that in all cases of the disease, whether acute or chronic, which have other than obviously local causes, both kidneys are usually found in essentially the same morbid condition, although the disease in one kidney is occasionally more advanced than in the other. Dr. William Budd and Mr. Paget have contributed to the 'Medico-Chirurgical Transactions,'* some interesting and valuable observations on the symmetry of diseases as an evidence of their origin in altered conditions of the blood.

[In the greater number of patients, the recovery is generally complete. One of the earliest indications of improvement is an increase in the quantity of urine, of a lighter colour, of less specific gravity, and less albuminous. During the convalescence from an attack of acute nephritis, an adult will pass from four to six pints of urine in the twenty-four hours: this will increase for several days, the urine being pale, and specific gravity as low as from 1010 to 1012. After an interval varying from a few days to a month, this quantity becomes gradually reduced to its normal amount and natural qualities.]

So long as the quantity of urine exceeds the normal measure, the gradually decreasing sediment is found to be composed of the *débris* of epithelium and blood, partly scattered and partly in the form of cylinders, which having remained in the tubes until they have become disintegrated are thence washed away in the current of liquid. There can be little doubt that the increased flow of liquid assists to clear out the tubes, and I have sometimes thought that possibly the epithelial *débris* in the tubes may exert a kind of reflex influence upon the Malpighian bodies, and thus excite them to pour out a stream of water for the actual purpose of flushing the tube, as a grain of dust on the conjunctiva excites a rush of tears from the lachrymal gland.

With reference to the process of desquamation, I have to guard against a possible misconception. It must not be supposed that the epithelial cells are dragged from the basement membrane merely by becoming mechanically entangled in the fibrinous effusion, nor that, when the cells are cast off, the basement membrane beneath is necessarily left uncovered by epithelium. One cell is pushed off by a new cell, which is formed beneath the old one, and, consequently, when the desquamative process has ceased, the tubes are still covered by an epithelial layer, as the skin by epidermis after cutaneous desquamation. It is only after a long continuance of the desquamative process that the renal tubes are left denuded in a manner which I shall presently describe. In a future lecture I shall have to refer to a form of renal disease which is characterised by the absence of desquamation, and I shall show that during the progress of this disease, although the fibrinous casts may be observed in the urine, yet they do not bring with them the epithelial cells, for the obvious reason, that as the cells are not thrown off by a process of desquamation, the fibrinous casts which pass over their surface have not the power of detaching them from the basement membrane.

[Besides these two modes, then, in which this disease may terminate, viz., in death and recovery, the disease may cease to be acute, the more urgent symptoms may have disappeared, and the urine may have become more abundant and less albuminous, and gradually merges into a chronic form.]

The next form of disease of which I have to speak is one which is characterised by a long-continued shedding of epithelium, which appears in the urine in a more or less disintegrated state,—a phenomenon which suggested the name of "chronic

* *Vol. xxv. See also* Mr. Paget's Lectures on Nutrition, *Medical Gazette.*

desquamative nephritis," as sufficiently expressing the nature of the disease. The tubes of the kidney generally lose their epithelial lining, and subsequently become atrophied, or they may be filled with a new and frequently an unorganised material; or, lastly, they may continue to be nourished, secreting serum into their cavities, and sometimes growing into cysts, which at length become visible to the unaided eye. Meanwhile, the renal blood-vessels undergo changes which are of great pathological interest and importance. The kidney in the advanced stages is commonly, but not invariably, much wasted, its substance firm, and its surface irregular. The urine is generally albuminous; its quantity and specific gravity are variable, but the former usually greater, and the latter less, than in health. The disease is so frequently associated with chronic gout, that Dr. Todd has proposed to call it the "gouty kidney." It not uncommonly, however, occurs unconnected with gout or the gouty diathesis. It sometimes has its origin in an attack of acute nephritis. A labourer is exposed to cold and wet, working, perhaps, for several hours in his wet clothes; he is seized with rigors; the urine becomes suddenly scanty, high-coloured, albuminous, and bloody; and more or less of general dropsy appears about the body. The attack compels him for a time to discontinue his work, and to seek for medical aid; the more urgent symptoms soon subside, but the urine continues to be albuminous. Believing himself to be well, or, it may be, impelled by the necessities of his family, he returns to his employment; and so a disease, which by judicious care might have been entirely removed, becomes a permanent chronic malady, and ends in fatal disorganization of the kidneys.

The essential character of the chronic desquamative disease is a shedding of the secreting cells, which are continually washed away in the urine, sometimes in an entire form, but generally in a more or less disintegrated condition. The urine, after standing, deposits a rather dense whitish precipitate, which is composed of cylinders of disintegrated epithelium which have been moulded in the tubes,—"granular epithelial casts." Some of the same disintegrated material is also irregularly clustered or scattered over the field of the microscope.

[An abundant secretion of urine is one of the most remarkable symptoms of the chronic desquamative disease; but it is most important to distinguish between a copious secretion and frequent micturition, the latter, and especially nocturnal micturition, being a frequent attendant upon all forms of renal disease.]

The amount of albumen varies considerably at different periods of the disease, being usually most abundant in the middle periods, and least so in the early and latest stages. The comparatively scanty secretion of albumen in the early stages is explained by the slight degree of vascular congestion at that period; and its diminution, again, in the last stage, is a consequence of the diminished vascularity of the kidney.—*Med. Times and Gazette, April* 3, 1852, *p.* 335.

56.—ON THE MICROSCOPIC CHARACTERS OF THE URINE IN BRIGHT'S DISEASE OF THE KIDNEY.

By John D. Macdonald, Esq., R.N.

Admitting the faithfulness of the exposition given by Dr. Bowman of the minute anatomy of the kidney, as demonstrable at any moment, a vindication of it may be called for. The pathology of the granular kidney seems to be simply this—an error of secretion, depending most probably upon the faulty character of the plasma from which it is derived, is attended with a gradually increasing deposition of oily matter within the cells, which are the principal agents in the process. This circumstance gives rise to an over-distension of the tubules, (more especially between the meshes of the matrix, where there is least resistance,) and thus, by mechanical pressure, the return of blood through the venous plexus which surrounds them is obstructed, the Malpighian tufts become congested, and may be ultimately ruptured; so that the presence of albumen in one instance, and actual blood in the other, may be detected in the urine. In some cases, the hemorrhage which occurs in this way is truly alarming, and the more immediate cause of a fatal issue. Notwithstanding the close analogies existing between the liver and kidney with respect to structure, function, and the diseases to which they are subject, there is this much

to be said, that the kidney does not admit of congestion with impunity, while the liver may be gorged to a considerable extent, and yet suffer comparatively little disorder of function. There may be often some difficulty in determining the fatty nature of the contents of the epithelial cells of the kidney when discharged with the urine, more especially if little is understood about the changes which they are liable to undergo, in obedience to common laws. The most usual metamorphoses of these cells exhibit eight phases. In the first, distinct oil globules are to be seen within them, varying in number and size, and giving a more or less rounded figure to the cells themselves, which become enlarged in a proportionate degree. 2ndly. The oil globules may run together, and so completely fill the cells, that (with the exception of the nuclei, which are generally visible) they appear like starch granules or little masses of oil, refracting the light powerfully. 3rdly. The fat gradually assumes a more concrete form, loses much of its brilliancy, and presents a mealy surface, any remaining globules becoming angular and irregular in form. 4thly. This concrete mass accumulates around the nucleus, and a clear space begins to appear between it and the cell-wall on the opposite side, which may be partly due to the imbibition of water. 5thly. An analytical change takes place in the fatty matter which separates into its two constituents, the more fluid part exuding through the cell-wall, while the more solid presents a crescentic appearance, the nucleus occupying its centre. 6thly. The cell itself breaks down at its weakest part, and the crescentic mass exhibits a well-defined convex border, formed by the remaining portion of the cell, and an uneven concavity where the cell is defective and the concrete fat is exposed. 7thly. The little crescent continues to extend itself, the outline becomes better defined, and a fusiform figure is ultimately assumed, the nucleus still occupying the centre of the mass, having undergone no apparent change; and 8thly. The pointed extremities of this little fusiform body are drawn out into filamentous processes, so that when several of these altered epithelial particles unite end to end, as often happens, one is reminded of the mode of development of the white fibrous tissue; and on the whole they present such a close resemblance to cancer cells, that the history of their formation is of some importance to be borne in mind with reference to diagnosis, especially if blood globules be also present with them; for it has been asserted, that if fusiform cells and blood discs are found to co-exist in the urine, certain indication is afforded of cancerous disease, affecting either the kidney or the bladder.

Now, many persons, keeping this doctrine in view, may arrive at once at a conclusion, without waiting to draw the distinction between an actual cell and a cell-like body. Forms very similar to those above described abound in the spleen, with blood discs seemingly as nuclei, and differing altogether from the muscular fibre cells (so called) of the trabecular sheaths.—*Med. Times and Gazette, March* 20, 1852, p. 289.

57.—ON BRIGHT'S DISEASE.

By Dr. H. Bence Jones, F.R.S.

[Dr. Jones looks upon the fibrinous cylinders which are found in the urine in this disease to be produced by congestion, causing effusion of liquor sanguinis, and thus fibrin and albumen being effused together, the fibrin coagulating in the ducts producing the cylinders, and the granular matter within them. When these appear in the urine, Dr. Jones always suspects the presence of granular disease of the kidney. Dr. George Johnson was the first who attempted the solution of the problem between this deposit and granular degeneration. Dr. Johnson subsequently extended his views, and distinguished four conditions of the kidney: 1. Acute desquamative nephritis. 2. Chronic desquamative nephritis. 3. Simple fatty degeneration. 4. Fatty degeneration with desquamative nephritis. He imagined that the microscope would enable us to distinguish the sediments peculiar to each of these several diseases, but the sediment unfortunately in the same case varies exceedingly at different periods. Sometimes even microscopic examination fails altogether when the chemical evidence has been decided enough. Hence says Dr. Jones:]

I cannot agree in the statement, that the microscopic examination of the urinary

deposit enables us to decide on the presence of different kinds of degeneration of the kidneys. On the contrary, it appears to my observation, and to my reason, far more probable, that in Bright's disease there is but one degeneration of the kidney, and that different states of congestion and inflammation give rise to the different microscopical appearances of the urine.

Take, for example, the phenomena observed after irritation of the skin, or inflammation of the pleura. First, congestion, without effusion or deposit; then, when the irritation is rather more, the congestion increases, and a watery blister or effusion forms, which may contain no trace of fibrin. If this fluid passed off, mixed with the urine, we should find that the urine was albuminous, without any trace of fibrinous casts or granular deposit. Suppose the congestion to be still more, the blister or the pleura then would contain, not only albumen, but fibrin also. The liquor sanguinis, which consists of fibrin dissolved in the albumen, would be poured out. If this occurred in the kidney, instead of in the skin or pleura, fibrinous casts would appear in quantity proportioned to the extent of the congestion, and the duration of this deposit of fibrin and albumen would depend on the duration of the congestion. Still greater congestion, or a more violent attack, would lead to the effusion of blood globules; and when a further stage of inflammation supervened, pus globules would be found, in addition to the fibrin and albumen. Obstruction is the consequence of congestion and inflammation, and, from whatever cause the obstruction may arise, it causes fatty matter to accumulate; and thus the deposit of fatty matter frequently, if not always, is the consequence, and not the cause, of the degeneration.

Let me, for a moment, take you to another disease of the kidney, (differing from Bright's disease,) in which I am certain that a greater or less degree of congestion does produce different deposits in the urine. In this complaint, I am certain, if you subdivided the disease according to the appearances in the urine, you would greatly err.

In the so-called chylous urine, occasionally, for many days, no blood globules, no trace of fibrin, nor fat even, may appear; but chemical reagents may show that albumen is present, and that the quantity of albumen in the urine is increased after exertion.

In the same case, at a different period of the disease, in addition to the albumen, fibrin will be present in the urine. It may be to so great an amount, that the bladder will be full of coagulum; and, at another time, to so small an amount, that only after the urine has been made for an hour or more will a fine coagulum be seen to form. If still more congestion of the kidney is present, blood globules in considerable quantity will appear, and their different deposits will alternate or be simultaneous, according as the congestion varies. It appears to me to be very probable, that in this so-called chylous urine the mammary processes of the kidney may be the seat of the congestion; while, in Bright's disease, it is more probable, from the deposit in the cortical structure, and from the appearances seen when the kidney is injected, that the secreting structure is the congested part.

I cannot, then, at present admit, that there are different diseases confused together under the denomination of Bright's disease; but I consider that the different appearances in the urine and the different appearances in the kidney arise from the same disease in different stages and states; sometimes in an early stage, at other times in a far advanced stage. Now, in one stage, in a state of the slightest possible congestion or inflammation; then, in the same stage, in a state of such congestion or inflammation that extravasated blood or purulent matter may be found mixed with the degenerated structure.

If you ask, first, in what does this degeneration consist? and, secondly, where is it seated? I am unable at present to give you a perfectly satisfactory answer. To the first question I reply, in chronic inflammation; and to the second, in the capillary vessels which surround the tubes. In other words, I consider that Bright's disease corresponds to chronic pneumonia, and not to chronic bronchitis. Even in the state of inflammation which follows scarlet fever, I cannot consider that there is an affection of the urinary tubes consequent on the desquamation resulting from the removal of the poison of scarlet fever by the kidneys. I cannot consider that the elimination of an unknown poison is the cause of this or of other affections which at the end of scarlet fever are apt to arise—for example, inflam-

mations of the eye and the ear. If the blood is in a state of increased fibrin, even when the blood globules are diminished, inflammation will most readily arise, and the slightest cause that produces a congestion here or there commences a local inflammation.

[With regard to the treatment of pericarditis, the special point to be attended to is, that no mercurial has been ordered.]

The intense action of mercury in Bright's disease has so frequently and so forcibly been presented to my notice, that I have formed a general rule never to give mercury in cases of albuminous urine. I have seen the most violent salivation from single doses even of grey powder, and on this account, even in serous inflammations consequent on Bright's disease, I advise you not to give mercury. Why mercury should act so energetically in this disease is probably explained by the altered state of the kidney. In health, most probably, mercury no sooner passes into the system, than it begins to pass out again by the kidney, like hydriodate of potash or the salts of iron. For example, if a single grain of hydriodate of potash is taken in an ounce of distilled water, in twelve minutes it can be detected passing off in the urine; but, if the kidney is diseased, then most probably this rapid excretion does not so easily take place, and the intensity of the action of this and other medicines may thus be greatly increased.

In the treatment of urinary poisoning, I know no single remedy which has produced an effect at all equal to that of a blister on the back of the neck. This appears to me to indicate that there is some local congestion of the brain, as well as some poison in the blood. I have as yet had no good opportunity for trying what the free administration of vegetable and mineral acids can do when head symptoms are present; in one case, large quantities of lemon-juice appeared to have a beneficial effect. The theory of Dr. Frerichs indicates the acid treatment, and it is worthy of a fair trial.

Dr. Watson is, without doubt, right in saying, that these head-symptoms may arise from insufficient supply of blood. Just, as in children, symptoms resembling hydrocephalus may arise from anæmia, when, instead of depletion, counter-irritation and stimulants are required. On this theory, no poison is considered to be present in the blood. The truth, probably, is, that too little blood, too much blood, and poisoned blood,—each of these states, in different cases, is the cause of the head-symptoms in Bright's disease.

As I cannot make any separation of Bright's disease into different diseases, so I am unable, from observation, to find that different kinds of treatment are desirable. The indications are, first, to relieve the congested kidney; and, secondly, to improve the general health. Gentle but decided action on the skin and bowels appear to me best to fulfil the first indication; cupping, vapour-bath, and elaterium, are the three most energetic remedies; cream of tartar, compound jalap powder, and abstinence from stimulants, are the milder agents. To improve the health, proper air, exercise, food, and small doses of iron, are essential.

Specific treatment by gallic and nitric acid has not benefited my patients in St. George's Hospital. In so-called chylous urine, I know well the use of gallic acid, in doses of two drachms daily; but, in Bright's disease, when ten-grain doses were given, I have more than once known bad symptoms to occur. In one case, violent convulsions made their appearance soon after the gallic acid was taken. In no case have I found that it stopped the albumen from passing away. I may here mention, that tannic acid appears to affect the stomach much more than gallic acid does.

With regard to the treatment by nitric acid, which has been extensively tried in Germany, I have not found any good result, even when dilute nitric acid has been taken daily for many weeks.

As to the result of treatment, omitting the cases which follow scarlet fever, which constantly recover perfectly, temporary relief is generally all that is obtained. Life may be prolonged for years. Even with ascites, I have known a patient in and out of St. George's Hospital for ten years. Usually, although every complication may be removed, the albumen continues in the urine. The patient resumes his work, and considers that he is recovered; but heat and nitric acid *show that this is not* the case; and it is only by great care that the tendency to a

return of the former symptoms, or to gout, can be prevented.—*Med. Times and Gazette, May* 8, 1852, *p.* 457.

58.—ON ALBUMINOUS URINE.

By Dr. H. Bence Jones, F.R.S.

[In his previous lectures Dr. Jones had noticed, that, in health, some substances are passing out in the urine which are so soluble in water that they never occur in the solid form. He now adds to these soluble excretions two others, namely, albumen and sugar. These never, like the others, become insoluble, and form calculi; they cannot be seen in the urine, unless by chemical reactions they are made to appear, and are always found only in states of disease. In certain states of disease, these substances, after entering into the blood, instead of undergoing those changes which they undergo in a healthy state of the body, through some defect or interruption, are thrown out of the body in the same state, or nearly the same state, in which they entered it: in the one case albumen, and in the other sugar; the two diseases, however, not having the slightest analogical resemblance between them. On entering into detail upon the test of albumen, Dr. Jones says,]

It does not exist in urine in a state of health, because if I boil any specimen of healthy urine, and acidulate it, I shall get no trace of a precipitate. Heat alone will give no precipitate, unless it happens that the urine is neutral, or only slightly acid. You will remember, that when the acidity is slightly removed, so that the urine approaches towards alkalescence, it will always give a precipitate by heat. Moreover you will remember, that in that case the precipitate disappeared on the addition of a drop of acid; but a precipitate of albumen, formed by heating it, will not easily disappear. On the addition of a drop or two of acid, it will remain as thick as before. Healthy urine, then, never gives a precipitate by heat which is insoluble in acid. Any urine whatever that gives a precipitate by heat which cannot be re-dissolved by a drop of acid, contains albumen. But to make still more sure of this point, there are other tests to be used in confirmation of your experiments. The errors which result from relying on a single test are well illustrated in the example of albumen. How many mistakes have resulted from trusting to heat alone, or to acid alone. With nitric acid alone a precipitate may be obtained. I showed you one example in the precipitation of urate of ammonia, and I may show another instance, in which, by the addition of this acid alone to the urine, an effervescence will take place, and in a few moments the liquid will become perfectly thick. This arises, not from any deposit of albumen or urate of ammonia, but simply because urea is present in excess, and the nitrate of urea crystallises out and forms a precipitate. This is likely to occur; but I have known this precipitate mistaken for albumen; and, as it serves to illustrate the necessity for using a double test for albumen, and the danger of trusting to any single test, it is mentioned here. Let me show you how the application of heat proves that this is not albumen. Rapidly effervescence ensues, the urea is destroyed by the nitric acid, and the fluid becomes clear; so also when nitric acid gives a precipitate of urate of ammonia, or urate of soda. Here is a specimen of urine which, with a drop or two of acid, will give a precipitate; but it will not remain permanent. On heating it, the precipitate will be rapidly re-dissolved, being decomposed into free uric acid, and nitrate of soda, or nitrate of ammonia. A permanent precipitate, then, with heat, and with acid, constitutes a good and a ready test for albuminous substances. Many additional tests have been given; and, as some of them may be occasionally used for minute quantities of albumen, I may show you the most remarkable. If I take a small portion of urine which contains albumen, and add to it a little acetic acid, I get no precipitate—acetic acid cannot coagulate albumen like nitric acid; but if I add also a little ferrocyanide of potassium, I get a plentiful precipitate. The urine must be acidulated, to make sure that the precipitate consists of albumen. Alkaline urine would give a precipitate with ferrocyanide of potassium when no albumen was present. Here is another beautiful test for albumen, which I have already brought before you, but the experiment is worth repeating. If I take urine containing albumen, and add to it a little sulphate of copper, and then an excess of caustic potash, I get a beautiful blue colour, which rapidly becomes purple. If I heat this mixture, it soon assumes a much more red hue. It will

not rapidly reduce the oxide of copper. I have shown you how with this test we can determine the presence of sugar by a rapid reduction. In the case of albumen, however, the oxidation is very slow. I have here a portion which I acted upon this morning; and though it has been standing for some hours, there is no remarkable reduction of the oxide of copper. Here is also another test, and which for very small quantities of urine is useful. If a single drop of healthy urine is placed in a watch-glass, and left to stand till it is evaporated to dryness, and this is constantly the case in microscopical examinations, on the glass where the object is placed,—if the urine contains no albumen, you will find that the glass can be cleaned with the greatest ease; the whole of the residue can be removed without the slightest trouble. But, if there is the least trace of albumen, when the water is gone off, and all that is volatile has escaped, the albuminous substance is so strongly adhesive, that it will form a small film upon the glass, which no rubbing can remove. I have here a glass with such a residue upon it, and I cannot remove it without scraping it off, or re-dissolving the albumen in water. Before I leave this subject, let me show you one other re-action of albuminous urine which is important, because it often leads to mistakes regarding the presence or absence of albumen. The usual method of examining urine is by the application of acid and heat to two different specimens of urine. Nitric acid is commonly used, because it gives a more marked precipitate than the other mineral acids. Not unfrequently, when a precipitate falls, the contents of the tube are thrown away and another portion of the urine is taken to which heat is applied. Let me show you the effect of using a different specimen for the purpose of determining the action of heat. Here is a specimen of albuminous urine, which is coagulable by heat, and on adding nitric acid the albumen falls; but if I throw it away, and examine another portion in the same tube, and then apply heat, the urine will not coagulate as before. The only difference is, that the one tube was clean and the other dirty. In the one tube there was no trace of nitric acid; in the other a very small portion of nitric acid was left; and this small quantity of nitric acid prevented the urine from coagulating on the application of heat. In St. George's Hospital this has occurred to me over and over again. I have examined urine, and said there was albumen in it; and others have come to me a little time afterwards, assuring me that there was no albumen present at all,—on boiling the urine they got no precipitate. The reason was, that the tube in which they tested the urine was not a clean one. They not only had albumen in the urine, but they also had a little nitric acid, and thus a compound of nitric acid and albumen was formed. The minutest trace of nitric acid is sufficient to combine with the albumen, and to change its properties, and to prevent the coagulation by heat. If I add an excess of acid, however, I shall get a precipitate, because nitrate of albumen is insoluble in dilute nitric acid, and falls as a precipitate. Alkalies also hinder the coagulation of albumen by heat. I have here some urine which I will make highly alkaline. You see it darkens in colour, as albuminous urine always does on the addition of alkali. If, then, I boil this highly alkaline urine, I shall get no coagulation by heat; I might boil it for an unlimited time without getting any precipitate. Thus, then, both acids and alkalies can interfere in the important reaction of heat on albuminous urine; they can both hinder the coagulation of the albumen. Practically, it is very important that this should be known, for if urine should be highly alkaline from carbonate of ammonia, or from fixed alkali, or if it should be intensely acid, it may not give any precipitate by heat, although albumen may be present in considerable quantity. So that the perfect test for albumen consists, as I have said already, not in heat alone, nor in acid alone, but in the joint action of the two re-agents upon the same specimen. If you are unable to use more than one, then the surest and most delicate test is, that of the nitric acid. If it give no precipitate, you may be sure, that very little, if any, albumen is present; and if it give a precipitate, it is most probable that albumen is present. To be certain, heat must be applied. If the precipitate be permanent on boiling, you may be sure that albumen is present. When very minute quantities of albumen are present, heat should be applied first, and a drop of acid should be added afterwards.

Having spoken at such length upon the tests for albumen, I must say a few words—and they must be but few—upon the diseases in which this substance is *accustomed* to appear in the urine. Albumen for the most part occurs in patients

who either have disease of the kidneys, or who have blood in the urine: the former alters the secretion entirely, generally making it much more dilute or watery than it otherwise would be; and the latter can frequently be seen by the naked eye. If the blood, however, is not visible by the naked eye when the urine is first passed, yet after it has been allowed to stand for some hours, the blood-globules may fall down and may become apparent, as for example in the specimen before you. When the urine is fresh, the blood-globules keep their perfect form, and in the course of twelve hours fall as a distinct sediment, forming a deposit from the urine; the red globules can be seen as a very minute and delicate layer at the bottom of the glass or bottle. The preservation of the globules depends upon the saline constituents of the urine hindering the action of the water upon the envelopes. If I mix blood-globules with distilled water, the pure water will be found to act most potently in dissolving them; but, in water containing salts in urine, or salt water, the globules will fall down without being acted upon. The salts in the urine, as in chlorides, the phosphates, and sulphates, of which I have spoken, hinder the action of the water, and allow the blood-globules to be detected with ease with the naked eye, or with the microscope. The globules can of course be less readily detected by the naked eye than by a microscope; such a microscope as I showed you in my lecture on the oxalates, is quite sufficient for the purpose. The most distinctive mark for determining whether the cortical structure of the kidney is affected or not, is founded on the microscopic appearance of the fibrin. In cases of hemorrhage from the kidney the fibrin appears in granular patches. Thus, in Bright's disease, it assumes peculiar forms or casts, depending upon the place of its effusion. Such urine is generally of a low specific gravity; it contains a slight sediment, which, when examined, consists of epithelium, mucus, fatty granules, and fibrin. This last has a peculiar form and shape. Instead of forming granular masses, it is moulded in the ducts of the kidney; the fibrinous moulds are often full of granular matter, sometimes of nucleated cells, sometimes of blood, sometimes of crystalline substances, as uric acid, or oxalate of lime; the microscope alone can detect these substances, and it can do so with the greatest ease. Sometimes (though much more rarely) the urine is of an exceedingly high specific gravity. Here are two specimens, the one of low, the other of high specific gravity. If I add nitric acid to either there will be a plentiful precipitate; and this precipitate, when boiled, will not disappear, showing that albumen is present. One urine has three times the specific gravity of the other, one being 1030, the other being only 1010. The denser specimen has thrown down, by standing, a considerable deposit of urate of ammonia, that substance which is soluble by heat. So that if I heat this specimen of thick urine, the first effect will be that it will become clearer, because the urate of ammonia which was precipitated is soluble in warm water. If, however, I continue heating it, I shall, if the urine be not too acid or too alkaline, have a dense precipitate of albumen. The precipitate is now forming. That it does not consist of phosphates may be proved by adding a drop of hydrochloric acid, which would immediately dissolve the earthy phosphates. The quantity of albumen which is thrown out in the urine varies very considerably, even in the same case, at different times, varying from 2½ grains to 15 grains in every 1,000 grains of urine. The loss of albumen in twenty-four hours is from 1½ drachms to 3 drachms on an average; the extremes being 45 grains and 5 drachms. Albumen, you will remember, exists in the solids of the serum. The solids of the serum of the blood in Bright's disease (as I showed you in one of my tables in the lecture on the blood, No. 57, p. 113) are below the average amount, viz., 74·8 parts instead of 80 parts in 1000 parts of blood. Dr. Christison gives in one place 52 parts per 1000 blood. The reason why the solids of the serum are reduced in this disease is, that they pass out of the body. Every portion of the urine has a portion of the albumen of the blood passing away with it. As rapidly as food is taken to supply the albumen, so rapidly is it partly lost in the urine; thus the blood is impoverished, and at the same time, in consequence of the alteration in the structure of the kidney, the urea and uric acid accumulate in the blood instead of passing off in the urine. It is not impossible that other substances, as, for example, oxalic acid, may accumulate in the blood in this disease, and thus may cause the symptoms of poisoning which are so often met with. At least it is cer-

tain that urea is not possessed of very poisonous properties; probably not being much more poisonous than nitrate of potash.

I will now mention an instance of a substance closely allied to albumen, first observed by Dr. Macintyre, in the urine, and described by me in the 'Philosophical Transactions' for 1848. If I take healthy urine and albuminous urine, and add alcohol in equal quantity to the two specimens, the result will be very different. If I add alcohol to healthy urine, there is but a very slight precipitate, consisting chiefly of the sulphates, which are insoluble in dilute alcohol. The addition of alcohol to albuminous urine, however, gives a plentiful precipitate; the principal portion of the precipitate is albumen, but, of course, the sulphates are precipitated likewise. I have here, in a bottle, about six ounces of urine, passed by the patient whose case is described in the 'Philosophical Transactions.' The bottle was filled with alcohol, and you see the inordinate quantity of precipitate which was formed. The precipitate has all the appearance of, and might be mistaken for ordinary albumen, as far, at least, as precipitation by alcohol is concerned. I have here a portion of this peculiar matter which was collected on a filter, washed, and re-dissolved in water; so far it may be taken to represent the urine which was passed. Let me examine it, and see if it has the ordinary re-actions of albumen. On boiling there is no precipitate. You might say, that this arises from the liquid being too acid or too alkaline. By testing it, it might possibly be found to be slightly alkaline. But let me take another portion of the solution, and test it with nitric acid. I told you that, for the most part—and I guarded myself by saying for the most part—if nitric acid gave no precipitate, no albumen was present. I try nitric acid, and you see there is no precipitate formed. Thus, I may conclude, that as neither heat nor acid gives a precipitate, I have no albumen in this urine. I have, nevertheless, an albuminous substance, but it differs somewhat in its re-actions from ordinary albumen. After this liquid has been boiled, and nitric acid has been added, if it be allowed to cool, a precipitate will be rapidly formed. I will cool this rapidly by agitating the test-tube in cold water, and you see it becomes almost solid, and re-dissolves when heated. You may remember, that I showed you a similar substance existing in beef-tea,—a substance which gave no precipitate with heat or acid until it was cooled. I mentioned then, that I had found such a substance in a case of disease; and this is the case I alluded to. If I apply another test for albumen,—if I acidulate the liquid, and then add ferro-cyanide of potassium, you will see that I shall have a plentiful precipitate, indicating the presence of an albuminous substance. This substance, on analysis, differs in composition from ordinary albumen, containing rather more oxygen; it appears to be one of the oxides of albumen, and not albumen itself. It occurred in a case of mollities ossium, in which I believe that free chlorine was at the same time passing off in the urine; but I could not satisfy myself on this point until after the patient was dead. The patient's appetite was sometimes ravenous; and no wonder that it was so, for every thousand grains of urine, as the following analysis shows, carried off as much albuminous substance as if a thousand grains of blood had been taken away.

Analysis of the Urine in a Case of Mollities Ossium.

Water	= 890·7
Albuminous substance	= 67·0
Urea	= 29·9
Uric Acid	= ·9
Earthy Phosphates	= 1·2
Chloride of Sodium	= 3·8
Sulphate of Potash	= 2·1
Alkaline Phosphates	= 4·4
	1000·0

Here is a specimen of this substance purified by means of frequent washing, re-precipitation, and treatment with ether.

In cases of so-called chylous urine, albumen appears in the urine. I have here a specimen of urine made by a patient suffering from this disease. It was passed as

white as you see it—indeed, whiter; for, as it has been standing for some months, it has become a little darker by slight decomposition. In another portion of this same urine, you see the white matter has risen up into a curd. If I take a portion of this urine and treat it with ether, I shall find it contains a substance perfectly soluble in ether; for, by agitating the two together, the urine loses its thick appearance, and becomes very nearly clear. The ether rapidly separates again, carrying with it a great deal of fat, a slight deposit taking place. If I pour off the ether, and evaporate it to dryness, I shall get, not a crystalline mass, for the fat is not crystallizable, but a mass of fatty matter, such as you see in this specimen, which has been collected from many quarts of urine. I have here other specimens of the same urine, passed at different hours, in which no fatty matter is seen, nor can any be extracted by ether: but if I add nitric acid and apply heat, I shall find that there is plenty of albumen present. So, also, if I add nitric acid to the liquid from which the ether was poured off, I shall be able to show, by the dense precipitate which is formed, that, in addition to the fatty matter, there is also a considerable quantity of albuminous substance present. An interesting point, on which I was enabled to satisfy myself in this case, by drawing blood from the arm, was, that the blood did not contain any excess of fat whatever. The fat and the albumen only came at certain periods of the day; and if I kept my patient quiet, that is, if I made him lie in bed, or if I examined the urine early in the morning, before he got out of bed, I found comparatively healthy water, most commonly not containing the slightest trace of an albuminous or fatty substance. If I made him get up before breakfast, and walk about, he would pass urine containing no fat, but giving evidence of albumen in plenty, and sometimes also, so much fibrin that the urine would coagulate, not into a white mass like blancmange, but into a sort of jelly-like, brownish coagulum, consisting of fibrin, tinged sometimes with the colouring matter of the blood. If I made him take breakfast before exercise, the urine was milky, as in the specimen before you; but, by making him lie in bed, the water was more healthy; whilst by giving him much food, more especially fatty food, an increased quantity of fat appeared in the urine. I made many experiments as to the effect of food, exercise, rest, and medicines; these are recorded in the 'Philosophical Transactions' for 1850, but more perfect medical details are given in the 'Medico-Chirurgical Transactions' for the same year. The patient is an intelligent man, who keeps an accurate record himself of the appearance of the urine each time it has been passed since I first saw him a year and a half ago. There has been no trace of the disease for many months, except on last Christmas day. I had warned him that anything that gave increased violence to the circulation would bring the complaint back again; but after eating and drinking on Christmas-day, like other people, he came to me the next morning, bringing with him a specimen of his urine, which was milky. He was again put on the treatment which had proved efficacious before—gallic acid and rest, and again with perfect success; as much as a drachm or a drachm and a half, and even two drachms of gallic acid being given in the course of the day. Since Christmas the urine has been perfectly healthy, and I believe no fat or albumen will be found in it again, unless violent exercise is taken. With such exercise the feeble vessels allow these substances to pass through; but with rest and gallic acid the vessels are contracted, and thus the disease is checked.

The following Table of the influence of diet, of pressure by means of a strong belt, and of medicines, is taken from the 'Medico-Chirurgical Transactions,' and is worth repeating here:—

On the Effect of Diet and Medicines on So-called Chylous Urine.

	Chylous in Different Degrees.	Free from Chyle.
On animal food in 1000 observations	968 times	32 times
Vegetable food "	910 "	90 "
Pressure-belt loose "	667 "	333 "
Pressure-belt tight "	638 "	362 "
When matico was taken "	474 "	526 "
When gallic acid was taken "	17 "	983 "
After gallic acid was taken "	0 "	1000 "

The most interesting fact connected with this case of albuminous urine was the

occurrence of albumen only at certain periods of the day. It is very probable that this disease in a much milder form occurs more frequently than has been supposed, and possibly these are the cases in which gallic acid has been said to stop the albumen in the urine in Bright's disease.—*Med. Times and Gazette, Jan.* 3, 1852, *p.* 5.

59.—*Causes of Albuminous Urine.*—M. Ed. Robin lately read a paper on the above subject before the Academy of Medicine of Paris; we subjoin an abstract of the same:—In the normal state the albumen is burnt in the blood, and the nitrogenized residue of this combustion, viz., urea and uric acid, is eliminated by the urine. The combustion is, however, not so complete as not to allow some little albumen to escape with the renal secretion; but this albumen, besides being very small in amount, is somewhat different from the ordinary kind. M. Robin thinks that if during a sufficiently long time the albumen underwent in the circulation a much smaller amount of combustion than is habitually the case, it might pass unaltered into the urine, instead of being thrown off in the form of urea and uric acid. The author cites the following facts in support of his opinion:—

The urine becomes albuminous in croup, in complete ascites, and in cases of capillary bronchitis, with emphysema, accompanied by much dyspnœa; in pulmonary phthisis, especially when complicated by pneumonia and marked with difficult breathing; in gestation, when sufficiently advanced to occasion an habitual congestion of the kidneys, owing to an impeded abdominal circulation; and in such states of the system in which a very incomplete respiration causes a marked diminution of combustion. The urine is also albuminous in cyanosis, of whichever nature it may be; in affections of the heart when they exist in such degree as to keep the patients in a state of semi-asphyxia; and, of course, in such cases where an obstacle to the circulation of the blood, or of malformation of the heart, prevents the hæmatosis from being as rapid as under ordinary circumstances. The urine is likewise albuminous in idiopathic or traumatic lesions of the nervous centres, which cause a lowering of temperature, and thereby a marked decrease of combustion; in diabetes, a disease where very often a lesion of the nervous centre seems to be the origo mali; where the great abundance of sugar in the blood seems to be an obstacle to the combustion of albumen; and where finally the natural heat is lowered by one or two degrees with patients who are severely affected. The urine is albuminous in that kind of nervous exhaustion which characterizes the state of frame called lumbago, which exhaustion must be connected with a great diminution of calorification, and slow combustion. The urine is likewise albuminous in consequence of severe exposure to cold of a large surface of the body. Finally, Bright's disease, where the urine is always albuminous and anæmic, is especially attributed to many of the causes which have been above enumerated as capable of exciting the passage of albumen into the urine.

The author continues by stating that some useful data may be obtained from comparative physiology. As a general rule, the urine of the common mammalia and of birds contains no albumen. Among reptiles, on the other hand, the batrachia, so remarkable by the low temperature of their animal heat, yield urine in which albumen is always to be found. It now remains to be proved, says M. Robin, that the urine becomes albuminous under the influence of such agents as interfere in a marked degree with slow combustion. The author then adduces the following conclusions:—

When the activity of the combustion which takes place in the blood is too feeble to burn the whole of the albumen which, in the normal state, should be consumed in a given time, the general vitality is diminished, and thus more or less albumen is allowed to pass unaltered into the urine,—viz., just so much organic matter as escapes the transformation into urea or uric acid. The proportion of urea contained in albuminous urine should therefore be smaller than it is found in normal urine, and such is found to be the case in the following diseases, the only ones, according to the author, in which experiments have been made—viz., pulmonary phthisis, diseases of cerebro-spinal axis, extensive and acute bronchitis with *intense dyspnœa, and Bright's disease.—Lancet, Jan.* 24, 1852, *p.* 96.

60.—ON URÆMIA, OR URÆMIC INTOXICATION.

By Dr. Fried. Theod. Frerichs, from a Review of his Work on Bright's Disease, in 'The Brit. and For. Med. Review.'

[The above is one of the most original and important sections in Frerichs' monograph on Bright's disease of the kidney. He says]

A train of symptoms frequently arises in Bright's disease, due to the contamination of the blood with the excrementitious constituents of the urine. There are two forms of uræmia, acute and chronic. Early in Bright's disease, patients often complain of headache or of a confused sensation in the head; their eyes grow dull and expressionless; they are forgetful and indifferent; and slow and inactive in their movements. If the urinary secretion becomes more abundant, these symptoms diminish, or they may disappear entirely. In other cases they increase in intensity, the drowsiness passes into stupor: at first, the patient may be aroused by loud calling and speaking, and then he gives rational answers; subsequently, the coma is complete, and the respiration becomes stertorous. Delirium is an infrequent symptom; when it does occur, the patient will repeat over and over again the same word or sentence. Convulsions frequently precede death.

The acute form of uræmia commences suddenly, and manifests itself in the three following ways: by depression of the functions of the brain, by irritation of the spinal cord, and by both sets of symptoms conjoined.—In the first form, the patients sink suddenly into a state of deep stupor, out of which they are very soon unable to be aroused. The face is mostly pale, and the pupils immovable; in other cases there is circumscribed redness of the cheeks, the conjunctivæ are then injected, and the pupils small. The pulse ranges between 60 and 90; on the occurrence of coma, it usually increases in size and hardness. The respiration is sometimes stertorous, the character of the stertor differing, as Dr. Addison pointed out, from that in cerebral hemorrhage.—In the second form, convulsions occur suddenly, similar in character to those seen in eclampsia and epilepsy. The whole muscular system is usually affected. Consciousness is undestroyed.—In the third form, coma and convulsions are conjoined.

These acute forms of uræmia are usually the result of sudden suppression of urine, particularly in Bright's disease, from scarlet and typhus fevers.

Acute uræmia may be readily confounded with cerebral hemorrhage, hysterical convulsions, reflex spasms of various kinds, narcotic poisoning, typhus fever, &c.

Closely allied to the foregoing disorders of the nervous system, as consequences of uræmia, are certain affections of the senses. The most striking of these is loss of vision; *Amaurosis uræmica.*—Like coma and convulsions, this local nervous affection may be slowly developed, or it may manifest itself in a few days or even hours. The patient complains of a sensation, as though a mist lay before his eyes, which from time to time becomes denser. The only change perceptible to the physician is some sluggishness of the pupil. Landouzy affirms that amaurosis is one of the most constant symptoms of Bright's disease. Bright and Barlow noted its occurrence in 4 out of 37 cases.

The sense of hearing is affected in Bright's disease about as frequently as that of vision. There was singing in the ears and difficulty of hearing in 6 out of Bright's and Barlow's 37 cases, and in 4 of 41 cases observed by Frerichs; thus the sense of hearing was affected in 10 out of 78 cases.

During the convulsions, the pulse, on account of the disturbance of the respiratory movements, is accelerated, and at the same time it is often irregular; in the intervals between the attacks of convulsions, it resumes its normal rate and regularity.

A febrile disturbance (*febris urinosa*) closely resembling typhus fever in its general characters, is sometimes produced by uræmia.

The cessation of the symptoms of uræmia is usually accompanied by a profuse secretion of urine.

Vomiting is one of the most constant and early symptoms of uræmia. The vomited fluid is generally alkaline, and contains carbonate of ammonia; when acid, the presence of ammonia in the egesta is proved by the addition of liquor potassæ. Frerichs says, "I have frequently sought for undestroyed urea in the vomited matters, but always in vain." Further, in uræmia produced by extirpation of the kid-

neys, and injection of urea into the blood, Frerichs always found a large quantity of carbonate of ammonia in the vomited matters, but not a trace of urea. Stannius and Cl. Bernard also failed under similar circumstances to detect urea. Lehmann and C. Schmidt, however, obtained urea in the matters vomited in cholera; yet when the phenomena of uræmia, the so-called cholera-typhus, had commenced, Lehmann found carbonate and other compounds of ammonia. The resolution of urea into carbonate of ammonia in uræmic vomiting is not effected, Frerichs maintains, as Bernard and Barreswil supposed, in the stomach, but takes place in the blood, and within the vascular system.

Uræmic vomiting, by affording an exit from the blood for the products of the destruction of the urea, appears in some cases to prevent the development of severe nervous symptoms. A case of this kind was long since described by Christison. In the experiments of A. Bernard and Barreswil, after extirpation of the kidneys, large quantities of an ammoniacal fluid were poured out by the mucous membrane of the stomach and intestinal canal. The animal continued awake, so long as this secretion continued; directly it ceased, the symptoms of uræmic intoxication set in.

It is doubtful in what relation the diarrhœa, which occurs in Bright's disease, stands to the uræmia.

The older physicians frequently asserted, that in cases of suppression of urine, the breath and perspiration had a fetid urinous odour. Many modern observers have denied this. Frerichs says, that whatever difference of opinion there may be as to the existence of this odour, it is a fact, that when the symptoms of uræmic intoxication, coma, convulsions, &c., commence, carbonate of ammonia is mixed in considerable quantity with the expired breath, and that the quantity of the ammonia is in proportion to the intensity of the uræmic phenomena.

"I have," he writes, "repeatedly demonstrated the ammonia contained in the expired air of sick men, and of animals into whose veins urea was injected after extirpation of the kidneys; reddened litmus paper quickly turned blue in the air issuing from the mouth and nostrils; a rod moistened with hydrochloric acid produced, when held in the same air, a more or less thick cloud. Animals into the veins of which urea was injected, continued quiet and awake so long as the expired air was free from ammonia, but as soon as a rod dipped in hydrochloric acid produced a white cloud when held in the expired air, the disorders of the nervous system characteristic of uræmic poisoning manifested themselves."

Frerichs' own observations have not enabled him to say anything definite as to the state of the sweat in uræmic intoxication.

After death from uræmia, no lesion of structure of the central organs of the nervous system can be detected. The membranes of the brain and spinal cord are normal; the quantity of fluid in the ventricles rarely exceeds an ounce—i. e., is within the range of health. In four cases of amaurosis uræmica mentioned by Landouzy, in one recorded by Bright, and in one observed by Frerichs, the optic nerves and the visual apparatus appeared normal. The stomach, also, even when during life it has been the seat of severe symptoms, is usually found after death unchanged in texture. So the intestinal mucous membrane may be normal in appearance, when during life there has been profuse diarrhœa. The kidneys exhibit the lesions characteristic of one of the three stages of Bright's disease. The blood is sometimes firmly, at others imperfectly coagulated; Frerichs thinks that in all the cases of uræmia he has seen occurring spontaneously or produced artificially, it has exhibited a peculiar shade of violet. Christison, Jaksch, and Hamernjk, have observed cases in which the blood had an ammoniacal odour similar to that of decaying urine. Chemical analysis proves, Frerichs affirms, that the blood, in every case in which the symptoms of uræmia are present, contains carbonate of ammonia, and also, usually, traces of undestroyed urea. The quantity of carbonate of ammonia varies greatly.

The above are, according to Frerichs' researches, the most important facts known concerning uræmia.

The cause of the symptoms of uræmia has been generally sought in the retention of the constituents of the urine in the blood. Osborne considered arachnitis to be the cause of the coma and convulsions; Prevost, Dumas, and Henle explained *the occurrence* of these symptoms by a supposed collection of watery fluid in the

ventricles. Examinations after death refute these two hypotheses. Dr. Rees rejects the urea theory, because the symptoms and the quantity of urea in the blood bear no relation to each other—i. e., there may be much urea in the blood and no symptoms, and severe symptoms and little or no urea in the blood; and supposes that a certain thinness of the blood is the essential determining cause of the phenomena. But, Frerichs says, there is no striking hydræmia in the acute form of uræmia. The experiments of numerous observers have proved that extirpation of the kidneys is followed by death in from three to five days. Vauquelin and Segalas injected urea into the veins of animals, and the only result was an increase in the secretion of urine; they injected urine itself, and death ensued. Frerichs, as Bichat, Courten, and Gaspard had previously done, repeatedly injected from 20 to 40 grammes of filtered human urine, sometimes even with the addition of urea, without any ill effects resulting.

It is clear, from these experiments, that neither urea, uric acid, nor urine itself with its extractive matters and salts, occasions the symptoms observed after suppression of urine.

In support of the statement that an accumulation of urea in the blood does not necessarily induce the symptoms of uræmia, Frerichs refers to cases reported by Drs. Bright, Christison, Rees, in which that substance was obtained in considerable quantity from the circulating fluid, the patient being at the same time free from all nervous symptoms: and adds,

"My clinical and experimental experience are in complete unison with the observations of the physicians above referred to. Blood was drawn from a man, suffering from advanced granular degeneration of the kidneys, in consequence of an attack of pericarditis; it contained more urea than I ever saw in that fluid. The patient died of the pericarditis, without having manifested any symptoms of uræmia."

Dr. Bence Jones says, that urea is "probably not much more poisonous than nitrate of potash." He thinks it possible, that oxalic acid may accumulate in the blood, and thus may cause the symptoms of the so-called uræmic poisoning.

The following is Frerichs' own theory of uræmia. The symptoms of uræmic intoxication, he says, arise in consequence of the urea accumulated in the blood being converted by the agency of a suitable ferment, into carbonate of ammonia, while yet within the vessels. For the supervention, then, he adds, of uræmic intoxication, two agents are necessary—1st, an accumulation of urea in the blood; 2ndly, the presence of a ferment by the agency of which the decomposition of the urea may be effected.

If the urea, after collecting in quantity in the blood, be suddenly decomposed, then the symptoms are those of apoplexy; if its decomposition is effected more gradually, then the symptoms resemble those of typhus terminating in coma and convulsions.

With the causes which occasion the development of the ferment, we are, Frerichs says, but imperfectly acquainted. In the acute blood-disease—e. g., typhus, scarlet fever, and cholera—this agent is rarely absent. Slight febrile disturbance, as from exposure to cold, or trifling local inflammation, seems in some cases to give the impulse necessary for the destruction of the urea. In cases of Bright's disease which arise during pregnancy, the ferment is usually developed. Diet appears to exercise some influence on the decomposition of the urea. On this perhaps, Frerichs adds, depends the fact that the uræmic symptoms are more common from Bright's disease in England and Scotland, than in France and Germany.

The presence of the ferment is manifested only by its effects. Frerichs offers no other proof of its existence.

Frerichs likens individuals whose blood is impregnated with urea, to animals into the veins of which amygdalin has been injected; from the presence of this agent alone they suffered little inconvenience, but a single sweet almond taken into the stomach suffices to deluge the blood with prussic acid, and to cause death in an instant. Such is the novel theory of uræmic intoxication propounded by Frerichs. In order to demonstrate its truth, it must, he says, be proved—

1st. That in every case of uræmic intoxication, a resolution of urea into carbonate of ammonia takes place.

2nd. That the symptoms characteristic of uræmia can be produced by the introduction of carbonate of ammonia into the blood.

Two series of experiments are described by Frerichs, as offering the required proof.

"In the first series of experiments, a solution of from thirty to forty-six grains of urea was injected into the veins of animals, the kidneys of which had been previously removed. They remained for some hours perfectly free from convulsions. . . . . In from 1½ to 8 hours they became restless, vomited acid chyme, or a slimy yellow alkaline mass, according to the state of fulness of the stomach at the commencement of the experiment. At the same time that ammonia was perceptible in the expired air, convulsions supervened, which occasionally ceased and returned again, and gradually passed into stupor with stertorous breathing. In some cases, convulsions were absent, and then sopor and coma were the first symptoms. After death, which took place from 2½ to 10 hours from the time of the injection of the urea, ammonia in large quantity was found in the blood; the contents of the stomach emitted, in most cases, a strongly ammoniacal (urinous) odour, and contained much carbonate of ammonia; in one case only was it somewhat acid, and even then it contained ammonia. This basis was detected in the bile and other secretions. The stomach was usually injected, and of a dusky-red colour. The brain and its membranes were normal in appearance; and the quantity of fluid in the ventricles was not increased.

"In the second series of experiments, a solution of carbonate of ammonia was injected into the veins of animals. Convulsions, often very violent in character, instantly ensued, and stupor quickly supervened. The respiration was difficult, the expired breath was loaded with ammonia, and vomiting of bilious matters occurred. The stupor lasted for some hours, and ammonia was expired during the whole time. Gradually, however, the latter disappeared, and then, by degrees, the animals recovered their senses. When more carbonate of ammonia was injected, while the animal lay in a state of stupor, the convulsions and vomiting recurred, and the urine and the stools passed away involuntarily; after the lapse of five or six hours the ammonia again disappeared from the blood, and the animal again became lively."

Although death by uræmia is the natural termination, so to say, of Bright's disease, yet the fatal result is sometimes caused by other lesions—e. g., by inflammation of serous or parenchymatous structures, by sinking from vomiting, diarrhœa, dropsy, tubercular suppuration, asphyxia, &c.

The diagnosis of uræmia from *apoplexia cerebri*, typhus, gastritis, convulsions of various kinds, and narcotic poisoning, is to be made by a careful examination of the quantity and quality of the urinary secretion, the presence of ammonia in the expired air, and the symptoms of disease derived from the organ the functions of which are disordered; thus uræmic coma is distinguished from that dependent upon hemorrhage into the brain, by the absence of paralysis of the voluntary muscles, the more frequent and softer pulse, and the more rapid breathing. The character of the stertor, too, differs in the two. The early occurrence of delirium and coma, and the absence of the eruption and of enlargement of the spleen, aid in diagnosing uræmia from typhus fever. At the same time it must be borne in mind that an eruption closely resembling the mulberry rash of typhus is sometimes present in the uræmia which follows cholera.

Disturbance of the Functions of the Primæ Viæ, Dyspepsia, Vomiting, and Diarrhœa.—When vomiting occurs during the first stage of Bright's disease, Frerichs says it is due to irritation of the nerves of the kidney, being produced in the same manner that it is when calculus is contained in that organ. In the second stage, disorders of the stomach have their origin chiefly in congestion of the mucous membrane, the hyperæmia being, in a large proportion of cases, due either to the abuse of alcoholic liquors, or to a mechanical impediment to the circulation. Towards the termination of the disease, vomiting occurs as a consequence of uræmic intoxication; and it is in this class of cases only, that ammoniacal compounds are found in the egesta. This last form of vomiting is especially difficult to control by the means of remedial agents. Sometimes it continues so long, and so constantly, that the patient sinks. Obstinate vomiting was observed by Frerichs in *11 out of 42 cases* of Bright's disease; it occurred in one-sixth of Rayer's cases;

in 8 of 20 cases recorded by Bright and Barlow; and in 8 of Malmsten's 68 cases. Diarrhœa, on the whole, seems to be less frequent than vomiting; 5 cases only have been observed by Frerichs in which it was a prominent symptom. The diarrhœa itself is always injurious; it rapidly endangers life from collapse; and is restrained by drugs with difficulty. The dropsy usually increases, in spite of the large quantity of watery fluid poured out by the intestine. After death in these cases, the intestinal mucous membrane sometimes appears healthy; in other cases it is thickened, red, and ulcerated.

Pseudo-Rheumatic Pains.—In the course of Bright's disease, and especially in its latter stages when the changes in the blood are the most marked, pains referred to the muscles and fasciæ, rather than to the joints, are frequently complained of by the patient. These pains belong to the same class as those observed in persons the subjects of cancerous cachexia. Frerichs mentions the case of an individual who died from hydronephrosis, in which their severity was so great that the physician mistook the case for rheumatism. These neuralgic pains are most severe when anasarca is absent or trifling.

The passage of albumen and fibrin into the *tubuli uriniferi* is, Frerichs maintains, the starting point of all the other phenomena which accompany Bright's disease, not only the visible local changes of the parenchyma of the kidney, but also almost all the alterations in the composition of the blood, and the disorders of the various functions arising from the latter, being strictly referable to it.

As to the anatomical changes of the kidney, they are clearly explained by the passage of the plasma of the blood into the urinary tubules. The albumen passes away with the urine; the greater part of the fibrin coagulates in the tubules of the cortical substance, and remains in them till washed away by the fluid poured out from the vessels of the Malpighian bodies. The expulsion of these clots is attended by detachment of the epithelium coating the tubules; while the latter, stripped of their secreting cells, ultimately collapse. The Malpighian capsules experience similar changes; the fluid issuing from the glomeruli, when the tubules are closed by coagula, collects and distends the capsules. The fibrinous part of this fluid coagulates, forming a coating to the vessels which are subsequently obliterated; ultimately the fibrin breaks up into granular protein molecules and fat-globules.

The effusion of the blood-plasma is produced in Bright's disease, as in every other case, by over-distension of the vessels. Thus, hyperæmia of the kidney is the first stage; this congestion is quickly accompanied by exudation of fibrin into the urinary tubules. The larger the number of tubules filled with fibrin, the greater the impediment to the influx of the blood, and the larger and paler the organ; some of the distended tubules project above the level of the surface of the cortical substance in the form of fine granulations. These conditions are included in the second stage. The fibrinous exudation matter and the epithelium subsequently experience fatty metamorphosis. Thus, Frerichs says, are formed the fatty kidneys which have been figured as a peculiar form of disease, *steatosis renum*. In other cases, the epithelium-scales imbedded in the fibrin shrink and break up, the tubules being filled with their fragments. Thus arises, says our author, one form of Bright's disease which has been named *Nephritis desquamativa* by Todd and Johnson.

In time, the tubules, from which the coagula have been expelled, collapse, and those which are still distended project as granulations on the superficial and cut surfaces of the cortical substance. The greater the number of coagula detached, and the greater the number of the collapsed tubules, the smaller the kidney. The gland is atrophied. This is the third stage. The change in the composition of the blood, in which we have the key to the various functional disturbances, is thus explained. The continued loss of albumen and fibrin—from 77 to 387 grains daily—necessarily leads to hydræmia; on this depends the changes in the *habitus* of the patient, the tendency to dropsy, the disturbance of the secretion of the mucous membranes, the inflammatory exudative processes, &c. The occlusion and collapse of the tubules occasion the collection of the excrementitious constituents of the urine in the blood, in which we have an explanation of the coma, convulsions, &c.

The exciting causes, according to Frerichs, of the effusion of albumen and fibrin into the tubules are—1st. Impediments to the onward flow of blood in the

veins. 2ndly. Paralytic dilatation of the capillaries, which may be occasioned either by specific irritants, as turpentine, or by alterations in the composition of the blood—e. g, in scarlet fever, cholera, or by reflex action from the skin.

After stating his own views, Frerichs gives a critical survey of the current theories of Bright's disease.

Bright's disease is said to consist essentially in fatty degeneration of the kidneys. Canstatt, Gluge, Johnson, and Todd, support this view. The occurrence of albumen in the urine is explained by supposing that the distending secreting cells press on the capillaries between the tubules, and so cause congestion of the vessels of the Malpighian bodies. This theory, Frerichs considers untenable, because, among other reasons, fat exists in the kidneys of some animals without albuminuria being the consequence.

Dr. Johnson has lately advanced another theory to account for the presence of albumen in the urine in these and other cases, and also for general dropsy, and hypertrophy of the heart in Bright's disease. He supposes that blood, charged with urea, passes with difficulty through the capillaries generally, and so congestion, effusion of albumen and fibrin, and even rupture and escape of blood, ensue. When the cells of the kidney are filled with fat, or are thrown off from the basement membrane in large quantities, urea must necessarily accumulate in the blood. Hypertrophy of the walls of the small arteries and of the heart is the result of the impediment to the passage of the blood through the capillaries. Dr. Bright long since suggested the possibility of hypertrophy of the heart, which he found frequently conjoined with renal disease, arising in consequence of the altered quality of the blood, "so affecting the minute and capillary circulation, as to render greater action necessary to force the blood through distant subdivisions of the vascular system." At present this can be regarded as nothing more than an hypothesis.

Bright's disease following acute exanthemata, it has been maintained, depends on desquamation of the epithelium of the kidney. Drs. Johnson and Todd are the supporters of this theory. That exudation of fibrin is the starting-point in these as well as other cases of Bright's disease, Frerichs thinks, is proved by the urine containing from the outset of the disease fibrinous casts of tubes, in which the epithelium is imbedded.

Frerichs considers that Mr. Simon, when he affirmed Bright's disease to be subacute nephritis with cyst-formations, mistook dilated tubules cut across for newly-formed cysts.

Drs. Gairdner and Johnson had anticipated Frerichs' explanation of the foundation of Mr. Simon's theory.

A lesion of the kidney analogous to cirrhosis of the liver, Frerichs considers rare, although he admits its occasional occurrence.

Frerichs states that Bright's disease cannot be held to be blennorrhœa of the kidney, as some have asserted, because the urinary secretion is diminished at the commencement of the disease.

We may add to these theories two others—namely, that supported by Dr. Graves, that Bright's disease is primarily a functional disease of the kidneys. Dr. Bright himself leant to this opinion. "I consider the disease in its commencement," he writes, "entirely functional." And that of Finger, of Prague, and Dr. Walshe—namely, that the disease is primarily a blood-disease, of which the lesions of the kidney are the mere manifestation.

"The true explanation seems to be," says Dr. Walshe, "that the state of the blood prevents the kidney from acting properly on the elements it is accustomed to excrete, not that its own functional aptitude is at the outset seriously impaired; in other words, that in the commencement the renal cells are still quite able to separate urea, if healthily constituted blood were offered to them by the vessels."

Finger expresses his views thus:

"As in other blood-diseases, so also in Bright's, the blood is first diseased; and like other dyscrasiæ, so also that in question may terminate fatally before it has occasioned the diseased product—i. e., the degeneration of the kidneys."

That is to say, that a patient may die from the morbid state of the blood which constitutes Bright's disease before the kidney itself has become the seat of any *lesion—Dr. Walshe* appears not to have been aware of Finger's opinion.

With reference to diagnosis: the only unequivocal sign of the existence of Bright's disease, Frerichs maintains, is the presence of the so-called "casts of tubes" in the urine. As to the value of the presence of albumen in the urine as a diagnostic sign of Bright's disease, any disease which occasions mechanical hyperæmia of the kidney may give rise to simple albuminuria.

[In the treatment of Bright's disease, we must keep in view, 1st, the cause which produced and keeps up the disease; 2ndly, the local action in the kidney itself; and 3rdly, the secondary symptoms. We may control some of the known causes of this disease, as exposure to cold and moisture, and errors in diet,—the evil effects of profuse suppuration, &c. The hyperæmia of the kidneys, induced by disease of the heart and liver, may be diminished by aloetic and other purgatives. In the first stages of the disease we lessen the hyperæmia of the kidneys by general or local bloodletting or derivatives. Frerichs abstains from applying counter-irritants in the lumbar region. The secretions of the skin and mucous membranes are to be increased. Saline aperients are to be avoided, from their action on the kidneys. In the acute form, single doses of calomel, combined or not with jalap, rhubarb, &c., may be given. Subsequently senna, castor-oil, colocynth, and other drastic purgatives, are to be selected.]

If the flow of urine is not sufficiently abundant to detach and wash out the fibrinous coagula, then diuretics are to be administered. The seltzer and Vichy waters may also be taken with benefit. In the advanced stage of the disease, Frerichs recommends strongly tannic acid, for the purpose of restoring the tone of the capillary vessels of the kidney, and diminishing the amount of albumen poured out with the urine.

As it is to the alterations in the composition of the blood that the secondary diseases are due, so it is from the state of that fluid that the indications for the treatment of the consecutive symptoms are to be drawn.

The indications for the relief of anæmia and hydræmia, on which, directly or indirectly, so many of the secondary symptoms depend, are, to favour the formation of blood-discs, and to free the plasma from its excess of water. The former is to be effected by good diet, vegetable bitters, and the employment of iron; the lactate is the preparation of iron recommended by Frerichs; the latter indication is to be fulfilled by increasing the secretion of the skin and intestinal mucous membrane.

The acute anasarca, which commences at the same time as the kidney-disease, if the patient be strong and the febrile symptoms urgent, is to be treated by general blood-letting; under other circumstances it is to be combated by diaphoretics and aperients. Hydragogue purgatives are sometimes useful. The value of elaterium, Frerichs considers, has been abundantly proved by Bright and Barlow, Christison, and Malmsten. Purgatives should never be administered when spontaneous diarrhœa is present. The mildest diuretics only are to be employed. Christison found digitalis and cream of tartar of service; Bright used uva ursi and pyrola umbellata; Rayer, inf. rad. armorac. The contra-indications for the employment of diuretics are, great pain in the region of the kidneys, bloody urine, and far advanced degeneration. If these means fail in relieving the anasarca, then acupuncture or simple incisions are to be made; if ascites be present, then paracentesis abdominis may have to be performed. The dangers to be feared from these measures, however, are considerable—namely, gangrene of the skin, and peritonitis. One of the cases reported by Frerichs proved fatal from peritonitis, the consequence of the introduction of a trocar into the abdominal cavity.

The *secondary inflammations* are difficult to treat. When they occur early in the disease, and are detected at once, general bloodletting may be cautiously employed; usually, however, exudation to a considerable amount has taken place before the disease is discovered. In pneumonia large doses of tartar-emetic and acetate of lead, with opium or digitalis, are the remedies from which the most benefit is to be anticipated. In pleuritis, pericarditis, and peritonitis, Frerichs recommends, if the anasarca does not prevent, local bloodletting and derivatives, and internally calomel and small doses of tartar-emetic.

The highly philosophical researches of Dr. J. Taylor render it in the highest degree probable that mercury exerts no beneficial influence over this class of inflammation.

In the treatment of *uræmic intoxication*, the first object to be attained is the restoration of the urinary secretion. Mild diuretics are the best remedies for the accomplishment of the desired end. Should these fail, then hydragogues are to be employed. Little hope can be entertained of diuretics acting, in the advanced stage of degeneration of the kidney. The second object is to prevent the injurious influence of the carbonate of ammonia developed in the blood on the nervous centres. When convulsions have commenced, this indication requires our first attention. Hydrochloric and the vegetable acids, Frerichs says, are the remedies which naturally suggest themselves; they pass into the blood, and are excreted again, either in their primitive or an altered form, with the urine. At the same time the patient may be washed with vinegar, and enemata containing acetic acid administered. If marked symptoms of cerebral congestion are present, purgatives and bloodletting may be required.

Vomiting, consequent on irritation of the kidneys, is to be relieved only by treating the local affection; that from chronic dyspepsia, consequent on the abuse of spirituous liquors, or disease of the heart, by bitters, narcotics, and antacids. Uræmic vomiting is most obstinate. Christison recommends creasote. Narcotics, Frerichs says, are of no service.

Diarrhœa, when it occurs during the latter stages of the disease, is very obstinate. Frerichs has generally found relief follow the use of liq. ferri muriatis.

In *bronchial catarrh*, expectoration is to be favoured, if deficient, by senega, ammoniacum, &c.; if in excess, it is to be restrained by tannic acid, acetate of lead, muriate of iron, and other astringents. Alum is sometimes extremely useful.

When the disease is the consequence of pregnancy, it is a question in some cases whether premature labour should not be induced.—*Brit. and For. Med. Chir. Review, April*, 1852, *p.* 304.

61.—ON THE USE OF COLCHICUM IN BRIGHT'S DISEASE.

By Dr. J. M. Maclagan, Edinburgh.

A consideration of the physiological action of colchicum,—namely, its diuretic action on the kidney, combined with the property of increasing the elimination of urea, leads me to suggest the probability of its being found useful in some cases of Bright's disease. This I venture to offer as a mere theoretical speculation, which I have had little opportunity of submitting to the test of experience, and which, so far as I can find, has not been adopted in practice by any of the authors who have written on this subject. It is true that Dr. Prout mentions the use of colchicum in this disease, but only where the gouty diathesis is present. Irrespective of this state, I venture to suggest its employment.

The presence of *urea* in the blood appears to be one of the established phenomena in the advanced stages of Bright's disease.

In 1829, Dr. Bostock suggested that *urea* being deficient in the *urine* of patients labouring under Bright's disease, might be detected in the *blood*. He accordingly "sought for it in the serosity of several of Dr. Bright's patients, but could detect only a matter possessing peculiar properties, which seemed to approach to those of urea."

In the same year, Dr. Christison first detected this principle in the serum of the blood in several cases of Bright's disease. One case which he relates is as follows:—

The urine in this case, although not greater in quantity than natural, contained only a fifth of the normal proportion of *urea*. The action of nitric acid on the extract of serum produced a beautiful radiated mass of foliaceous pearly crystals of *nitrate* of *urea*.

In 1840, Dr. G. Owen Rees published a statement of his analyses of the blood of patients labouring under Bright's disease, showing a diminution in the amount of *albumen*, and an abnormal quantity of *urea* in the blood.

The following are his analyses:—

(1.)	Serum—sp. gr. 1015—contained in 1000 grains,—	
	Albumen,	46·980 grains.
	Urea,	0·209
(2.)	Serum—sp. gr. 1025—contained in 1000 grains,—	
	Water,	904·20
	Albumen,	65·00
	Extractive and salts,	30·30
	Urea,	0·50
		1000·00
(3.)	Serum—sp. gr. 1029, or natural—contained in 1000 parts,—	
	Water,	896·6
	Albumen,	81.6
	Extractive and salts,	21·3
	Urea,	0·5
		1000·0

In the first two cases there is a deficiency of *albumen;* in the last case, although the albumen is normal in quantity, we have morbidly present nearly 0·5 per mille of *urea*, for in health but a very slight trace of that substance can be detected.

Dr. Rees states the largest amount of *urea* he has found in the blood of Bright's disease is 0·5 per mille, and the smallest 0·209.

Dr. Bright states,—"In one very remarkable case, where the albuminous condition of the urine has constantly existed, as far as I know, from frequent experiment, for above three years, the quantity of urea in the blood is very considerable. The results of chemical analysis by Dr. Babington were, that the urine did not contain one-third of the *urea* which it does in health, while about 1 per cent. of *albumen* supplied its place. The serum of the blood was remarkably light, in consequence of its deficiency in albumen, having a specific gravity of 1021 instead of 1030; and the quantity of *albumen* in 1000 grains of serum, amounting, after careful drying, to only 50 grains; whereas from 80 to 100 parts in 1000 is the usual proportion in healthy serum; and it contained fully as much *urea* as the urine did, the 1000 grains yielding nearly 15 grains of that substance.

The following analysis of the urine in Bright's disease will show the relation which *urea* and *albumen* bear to each other and to the other constituents of the urine; but first the analysis of normal urine, as given by Becquerel, must be noticed. Specific gravity 1018·900.

Water,	968·815
Urea,	13·838
Uric acid,	·391
Inorganic salts,	7·695
Organic matter,	9·261
Total	1000·000

Compare now the following analysis of the urine in Bright's disease (Simon.)

	I.	II.
Specific gravity,	1014	1022
Solid constituents,	33·90	66.50
Water,	966·10	933·50
Urea,	4·77	10·10
Uric acid,	0·40	0·60
Fixed salts,	8·04	10.00
Extractive matters,	2.40	—
Albumen,	18.00	33·60

It will be seen that in the first of these the *urea* is only a third of its normal quantity, *uric acid* and *salts* nearly natural, while there is morbidly present four and a half times the amount of *urea*, of *albumen*. In the second, the *albumen* is in the ratio of three to one of *urea*.

From these statements it will be seen that in Bright's disease the *albumen* morbidly excreted in the *urea*, and the *urea*, are correlative and vicarious principles. That in the *blood*, while the *albumen* is diminished, *urea* is morbidly retained; and in the *urine*, while *urea* is greatly deficient, *albumen* is morbidly present.

Might we not then reasonably expect that colchicum (acting, as has been stated) would sometimes act as a favourable auxiliary in the treatment of this disease? Its use in the dropsy succeeding to scarlatina, would seem to corroborate this view. At all events, when *ascites* or *anasarca* are present in Bright's disease as intercurrent affections, the use of colchicum, both as a cathartic and diuretic, seems to me to be indicated; and in those cases where coma supervenes, from accumulation of urea in the blood, I am confident that it will prove of eminent service.—*Monthly Journal of Med. Science, Jan.* 1852, *p.* 30.

62.—*On the Modes of distinguishing Uric Acid from Urate of Ammonia Calculi.* By JOHN ZACHARIAH LAWRENCE, Esq.—[Mr. Lawrence was led to make the following observations, from the frequent problems which arose in his efforts at analysis, whether a given calculus was composed of uncombined uric acid, or whether this acid was in combination with ammonia.]

"1. Urate of ammonia is distinguished from uric acid by evolving ammoniacal fumes in treating the fragment of the calculus with caustic potash." This test is often not available in determining the character of the necessarily small quantities the analyst of calculi has to deal with; besides, uric acid itself, in common with other nitrogenised animal substances, might of itself evolve ammoniacal fumes on being treated with a concentrated solution of potash.

"2. An aqueous solution of uric acid does not give the murexide test, an aqueous solution of urate of ammonia does." This statement requires qualifying, in so far that, while this is true of cold solutions, it is not true of boiling solutions, which give the test with both the substances under consideration.

"3. A cold aqueous solution of uric acid yields no precipitate on the addition of hydrochloric acid; a similar solution of urate of ammonia does." This is perhaps the best of all the tests yet mentioned, though it is omitted in many books.

And, lastly, I have to add a test which, long applied as it has been to the recognition of urinary deposits, has not, as far as I am aware, been made use of in the analysis of urinary calculi. A small fragment of the calculus is reduced to a fine powder and boiled for a minute or so in distilled water, and a single drop of the solution, placed on a plate of glass, examined microscopically. If it be uric acid, a multitude of well-defined crystals make their appearance as the drop cools; the various forms of these crystals are well figured in Bowman's 'Medical Chemistry.' Should, on the contrary, the substance be urate of ammonia, no crystals are seen, but those amorphous aggregations of globules, generally of a brownish tint, so familiar to the examiner of urinary sediments, will be perceived. Even the naked eye will readily distinguish the glistening crystalline precipitate of uric acid from the dull amorphous one of urate of ammonia. It is only in solutions of urate of ammonia which have stood for some time that I have been able to detect crystals, which were then of an entirely different character from those of uric acid. In conclusion I beg to remark, that while this last test does not yield to any of the others in delicacy and precision, it will solve a problem which none of the others can do, namely, that of detecting mixtures of the two substances in question.—*Med. Times and Gazette, March* 27, 1852, *p.* 326.

63.—*On a New Method of obtaining Hippuric Acid in considerable quantity without Evaporation of the Urine.* By Mr. E. RILEY, Assistant in the Museum of Practical Geology.—The method described for obtaining hippuric acid consists in adding to fresh cow's urine some hydrochloric acid, in the proportion of half a fluid ounce to a pint of urine. The hippuric acid being very slightly soluble in this acid liquor, is deposited, together with some colouring matter, from which it is subsequently purified by dissolving it in water with excess of lime, adding animal char-

cool, and heating the mixture for half-an-hour or more. On adding hydrochloric acid to the filtered liquor, and allowing the mixture to cool slowly, the hippuric acid is deposited in large prismatic crystals. It sometimes requires to be further purified by repeating this process. The acid thus prepared, on being analyzed gave results agreeing with the established formula for hippuric acid, namely, C_{18} H_8 N O_6 + H O.—*Lancet, March* 6, 1852, *p.* 249.

64.—ON AMMONIACAL AND ALKALINE URINE.

By Dr. H. Bence Jones, F.R.S.

The differences between ammoniacal and alkaline urine are mentioned in the following table:—

Contrast between *Ammoniacal Urine*	and	*Alkaline Urine.*
Alkalescence from carbonate of ammonia.		Alkalescence from fixed alkali.
Caused by local disease.		Caused by general disorder.
Blue paper made red on drying.		Blue paper remains blue on drying.
Alkalescence is constant.		Alkalescence is occasional.
Excess of mucus and pus present.		No pus. Rarely much mucus.
Prismatic crystals generally seen.		At first granular deposit only seen.
The iridescent film has prismatic crystals.		The iridescent film consists of thin plates.

The alkalescence of ammoniacal urine is caused by carbonate of ammonia. It arises from local disease,—from some altered mucus setting up a change in the urea, and giving rise to carbonate of ammonia. When tested by blue paper it is made red on drying. The alkalescence is generally found to be constantly present for days or weeks together. The local disease consists in inflammation of the mucous membrane of the bladder, which gives rise to an excess of mucus, and frequently to pus. In the sediment, prismatic crystals are constantly seen. If the urine is left to stand, an iridescent scum forms on the surface, giving all the prismatic colours, and in the film distinct prismatic crystals can be found. But look at the difference when the urine is alkaline from fixed alkali. It is caused, as you see, by general disorder, I mean indigestion, which is not a derangement of the stomach alone, but a disorder of the whole system. When tested with blue paper, it remains blue when the paper is dried. The alkalescence is only occasional, lasting usually a few hours. No pus can be found, and there is rarely much mucus present. There are sometimes oxalate of lime crystals present; but these are very variable. In how many points does such urine differ from ammoniacal urine? the most striking difference is the absence of pus, and of an excess of mucus. But there are other points of difference still to be mentioned. In urine alkaline from fixed alkali, the precipitate which forms at first consists only of a granular deposit of phosphate of lime. Usually there is no appearance of prismatic crystals at all until the urine has stood for some time. The iridescent film, if you examine it with a microscope, will not be found to consist of prismatic crystals, but of fine thin plates of phosphate of lime, which are perfectly soluble in any acid, and have no crystalline appearance. If these facts are true, then what has been called the phosphatic diathesis, should be called alkaline urine; and this must be subdivided into two very different states, the one ammoniacal urine, and the other urine alkaline from fixed alkali; as the causes which produce these states are totally different, the practical importance of the distinction cannot be overlooked.

There is one very interesting fact connected with alkaline urine, which it is well that I should point out to you. Here is a specimen of urine which is alkaline, from fixed alkali. It was passed a few hours after a meal,—the time, you will remember, when the quantity of uric acid, is always greatest. If I test this urine, by adding to it a little hydrochloric acid, it would give a considerable and decided precipitate, because a considerable quantity of uric acid is in solution. The precipitate, most probably, is caused by the insolubility of urate of soda in hydrochloric acid. Not only does mineral acid cause a precipitate in this urine, but if I boil it

previous to the addition of the acid it will give a plentiful precipitate. This last precipitate consists of the earthy phosphates, and it is immediately dissolved on the addition of any acid. The precipitate with acid of which I first spoke consists of urates, and by heat the urates will be re-dissolved or decomposed, forming a much less perceptible precipitate of uric acid. Thus I have urine at a certain hour of the day which will not unfrequently give a deposit by heat which is soluble in acid, and at the same time it will give a deposit by acid which is soluble by heat. Diminished acidity of the urine and increased excretion of the urates give rise to these remarkable reactions. Such urine has often been wrongly called albuminous.

I have endeavoured to impress on you the fact, that an alkaline state of urine exists totally different, in my belief, from the state which results from inflammation of the bladder,—from that state which leads to these vast phosphatic concretions which depend upon the urine becoming ammoniacal. If the bladder returns to its healthy state the urine is no longer ammoniacal, and the immense deposit of triple phosphate and of phosphate of lime ceases. How different in this state from that of alkalescence from fixed alkali. In the one case the physician must direct his attention to the state of the stomach; in the other case, to the state of the bladder. In one, if the irritability of the stomach is removed the alkalescence ceases to appear; in the other, if the inflammation of the bladder is subdued, no altered mucus is poured out, no decomposition of the urea occurs, and no alkalescence from volatile alkali is produced, and the earthy phosphates are not precipitated.—*Med. Times, Dec.* 13, 1851, *p.* 609.

65.—ON THE ALKALINE AND EARTHY PHOSPHATES.

By Dr. H. B. Jones, F.R.S.

For many years the earthy phosphates which form calculi,—the phosphate of lime, and the phosphate of ammonia and magnesia,—were alone mentioned in medical works; but the three alkaline phosphates of which I have spoken occur in the body in much larger quantities than the phosphates of lime or magnesia. The phosphates which form calculi make themselves apparent when the urine becomes alkaline; but the alkaline phosphates never appear, being highly soluble in water, and never being precipitated by any other substance occurring in the urine; thus they have been overlooked until late researches on the subject. The earthy phosphates, like the uric acid and the oxalate of lime, force themselves upon the notice; while the alkaline phosphates are like the sulphates,—they never form calculi. The sulphates in the urine would never be seen but for chemical tests; and so with these alkaline phosphates; but for chemical tests we should never know of their existence in the urine. When the urine is alkaline, you can judge by the eye whether there is much earthy phosphate present; but of the phosphates of soda you can form no judgment whatever except by a chemical analysis. If I take a portion of healthy urine, and add to it any alkali which will render the earthy phosphates insoluble, they will of course fall as precipitates. These earthy phosphates—the phosphates of lime and magnesia—are excessively soluble in all acid liquids,—almost in the very feeblest acid. If I take acid phosphate of soda, and mix with it a little phosphate of magnesia or phosphate of lime, and then heat them, the earthy phosphate will be speedily dissolved by the acid phosphate of soda; and still more speedily would it be dissolved if hydrochloric acid were added, or any other acid stronger than the acid phosphate of soda. You will remember the test for calculi consisting of phosphate of lime or phosphate of ammonia and magnesia, mentioned in a former lecture, soluble in hydrochloric acid, without effervescence, with or without heat, precipitable by alkalies. If, then, I neutralise the acid re-action of the urine, I shall get a precipitate of phosphate of lime, and phosphate of ammonia and magnesia. If I take healthy urine, and add ammonia to it, you will see the precipitate which will fall. The liquid becomes cloudy. The ammonia has neutralised the acid re-action of the urine, and a precipitate is the result. It consists of the mixed earthy phosphates which were held in solution by the acid of the urine. If I were to filter this liquid, and then test it again for earthy phosphates, I should *find no more present*. But are there no other phosphates at all to be found

there? If I added any earthy matter to the liquid, after it has been filtered (chloride of calcium for instance), I should get another more distinct and more plentiful precipitate. When I precipitate the earthy phosphates, there is much less precipitate than when, after filtration, I add fresh earthy matter to throw down the rest of the phosphates. The two quantities precipitated represent the comparative quantities of earthy and of alkaline phosphates existing in the urine. If I fill a 1000 gr. bottle with urine, and precipitate it with ammonia, I shall get the earthy phosphates alone. If I take a 500 gr. bottle, (which in this case is more convenient, as the precipitate is so large), and precipitate the urine it contains with an earthy salt, such as chloride of calcium or magnesium, and also with ammonia, I shall get both the earthy and the alkaline phosphates precipitated together. Then by deducting half the amount of the first precipitate from that of the other, I clearly arrive at the comparative quantity of each in the urine. It is in this way that the numbers given in the following table have been ascertained:—

Amount of Earthy Phosphates and Alkaline Phosphates.

	Earthy Phosphates.	Alkaline Phosphates.		Urine.
Before food ...	0·40 gr. ...	7·56 gr. ...	sp. gr.	1028
After „ ...	1·45 ...	5·77 ...	„	1030
3rd day, Vegetable Food.				
Before food ...	0·37 ...	8·19 ...	„	1028
After „ ...	1·86 ...	5·56 ...	„	1032
3rd day, Animal Food,				
Before food ...	0·48 ...	5·06 ...	„	1025
After „ ...	0·81 ...	4·31 ...	„	1025

I deemed it important to determine whether the earthy phosphates, apart from the alkaline phosphates, did not undergo peculiar variations of their own, and to know what were these variations, on which, perhaps, the formation or rapid increase of the phosphatic calculi might depend. By precipitating the urine with ammonia alone, the variations in the earthy phosphates were determined; and these are represented in the table. I found that before food, urine with a specific gravity of 1028, contained 0·40. gr. of earthy phosphate in every 1000 grains of urine; but after food the amount was very much increased, being 1·45 gr. per 1000 grs. of urine; the specific gravity being increased to 1030. This was the result of many experiments made in the way I have mentioned. I also ascertained the influence of different kinds of food. With vegetable food, the third day there was 0·37 gr. of earthy phosphate in 1000 grs. of the urine before food, nearly the same as with mixed diet; but after food the quantity was more than on mixed diet, being 1·86 grs. per 1000 grs. of urine. With animal food, on the third day, there was 0·48 grs. before food; and after food 1·81 grs. per 1000 grs. of urine, not so high as with vegetable food. The result, then, shows that food, whether animal or vegetable, distinctly increases the amount of the earthy phosphates in the urine. After animal food the amount does not appear to rise so high as after vegetable food. There is no doubt, from these numbers, that the earthy phosphates are distinctly increased by food, and consequently the rapidity of the formation of a phosphatic calculus is increased by the same cause. Having determined the variations in the earthy phosphates in health, and with different diets, I went on to determine the effect of different diseases on the amount of earthy phosphates, in the urine; and it was with this object that the whole of the investigation of the variations in health was undertaken. I soon found, however, that I could draw no conclusions from such experiments, for the food had a much greater influence than the nature of the disease. I was thence led to inquire whether the influence of disease was more manifest in the variations of the alkaline and earthy phosphates together than in the variations of the earthy phosphates alone. I soon found that the earthy phosphates in the urine were present in much smaller quantities than the alkaline phosphates; and that conclusive results could only be obtained by determining the amount of the alkaline as well as of the earthy phosphate. I found that every 1000 grains of urine, containing before food 0·40 gr. earthy phosphate, contained no less than 7·56 grs. alkaline phosphate—18 times as much alkaline as earthy phosphate. After food, the earthy phosphate amounted to 1·45 gr., and the alkaline to 5·77 grs. per 1000 grs. urine. So with vege-

table food only. After living three days on vegetable food, the quantity of alkaline phosphate was greater than when mixed diet was taken; and it was also higher after vegetable than after animal diet. The alkaline phosphates, however, as you will observe, are not increased immediately after food, as the earthy phosphates are; on the contrary, they are less after than before food, in consequence of the earthy phosphates being increased at the expense of the alkaline phosphates. The variations of the earthy phosphates do not correspond to the variations of the alkaline phosphates, but only to the quantity of lime and magnesia which passes out by the urine at any time; for if any earthy salt is given as medicine,—a little chloride of calcium or sulphate of magnesia, for instance,—I find the quantity of earthy phosphates distinctly increased by it.

The following table shows you, that, when no chloride of calcium was taken, the quantity of earthy phosphates was 0·36 gr. per 1000 grs. of urine; but after a dose of chloride of calcium (there being no interference from food), the amount rose to 1·08 gr. per 1000 grs. of urine. With a small dose of sulphate of magnesia (40 grs.) the earthy phosphates rose to ·90 grs. per 1000 grs. of urine: and with a larger dose (120 grs.) they amounted to 2·99 per 1000 grs. of urine.

Effect of Chloride of Calcium and Sulphate of Magnesia on the Variations of the Earthy Phosphates per 1000 grs. of Urine.

		Sp Gr
38 grs. Chloride of Calcium before food	1·08 grs.	1022
No Chloride of Calcium	0·36	1027
40 grs. Sulphate of Magnesia	0·90	1029
120 „ „	2·99	1027

Thus the variations of the earthy phosphates are dependent on the variations in the amount of the earthy matter passing through the system. The quantity of lime and of magnesia going out by the urine is represented by the amount of precipitate obtained on the addition of ammonia to the urine. If I add a little more lime or magnesia, either by taking it as a medicine, or by pouring the solution into the urine after it has passed out of the body, the quantity of earthy phosphate may be increased at pleasure until no more alkaline phosphate remains. The knowledge of the variations of the lime and magnesia may be important, but such knowledge is totally different from that of the variations of the phosphates in the urine. If, then, you desire to trace the diseases in which an excess of the phosphates appears in the urine, it is absolutely necessary not only that the earthy phosphates in the urine should be precipitated by ammonia, but that some earthy salt should be added at the same time to precipitate the alkaline phosphates. I began my experiments with chloride of calcium; it would have been better to have commenced and continued with sulphate of magnesia, but as I used chloride of calcium first, I thought it advisable to continue it, so that there might be no variations arising from the form of the experiment. I precipitated the phosphates in the urine then, with chloride of calcium and pure ammonia, and thus the numbers were obtained which I brought before you in the third lecture on respiration. The effects of diet, exercise, and medicines, on the variations of the phosphates are detailed in the 'Philosophical Transactions' for 1845, 1847, and 1850. The variations which disease produced upon these phosphates I have already mentioned in the third lecture on respiration. It appeared that the amount of phosphates present was decidedly increased in diseases in which oxidation was going on in that part of the body which contains most phosphorus. In the brain, for instance, which contains sulphur in its albumen, and phosphorus in its fatty matter, when acute inflammation occurred, the phosphates and sulphates in the urine were greatly increased. The phosphates also were increased in cases of delirium tremens, examples of which have already been brought before you. I speak more particularly of the alkaline phosphates than of the earthy phosphates, these last occurring in such quantities as to be comparatively of much less importance than the former. The earthy phosphates are important only because they may form phosphatic calculi. What I wish to impress upon you now is, that the true phosphatic diathesis—that is, the occurrence of an excess of alkaline and earthy phosphates in the urine—may not make itself apparent to the eye. The alkaline phosphates may be present in *an inordinate* excess, and as in the sulphuric diathesis the sulphates may be *immensely increased,* and yet the eye may know nothing of the excess, so in the

phosphatic diathesis it requires, as I have said, that chloride of calcium or sulphate of magnesia should be added with pure ammonia, and then the total amount of phosphates becomes apparent.

The earthy phosphates, which alone form the phosphatic calculi, are precipitated, solely because the urine becomes alkaline. The formation of this kind of calculus, I repeat, depends upon the alkalescence of the urine, and not on the presence of an excess of earthy phosphates. But the true phosphatic diathesis is that in which the alkaline and earthy phosphates occur in inordinate excess in the urine, and this excess has no relation whatever to alkalescence of the urine; so far from it, that usually when there is the greatest excess of phosphates in the urine, a highly acid re-action is observed.—*Med. Times and Gazette, March* 27, 1852, *p.* 310.

66.—ON THE OCCURRENCE OF OXALATE OF LIME, AND ON THE VARIATIONS OF THE SULPHATES IN THE URINE.

By Dr. H. Bence Jones, F.R.S.

[Whence is the source of oxalate of lime in the urine; and what does it indicate? It has generally been held that oxalic acid came from our vegetable food, being one of the results of a want of a perfect and proper digestion, and that of the vegetable food—sugar more especially—was the source of the oxalic acid. Dr. Jones says:]

If I take a portion of sugar, (and a very small portion is sufficient,) mix it with a little nitric acid, and heat it, I have decomposition of the sugar produced; and the decomposition which actually takes place is an oxidising action,—some of the nitric acid is decomposed, and furnishes oxygen to the sugar, which is thus oxidised; and I have the results of that oxidation in the solution. On the application of heat, an intense action takes place, the liquid turns red, and rapidly evolves red fumes. The result is, that I have oxalic acid produced. The change which thus takes place I have represented in my diagram.

Formation of Oxalic Acid from Sugar.

Sugar	+	18 Oxygen	= Water	+ Oxalic Acid.
$C_{12}\ H_{12}\ O_{12}$	+	18 O	= $H_{12}\ O_{12}$	+ $C_{12}\ O_{18}$
Sugar	+	24 Oxygen	= Water	+ Carbonic Acid
$C_{12}\ H_{12}\ O_{12}$	+	24 O	= $H_{12}\ O_{12}$	+ $C_{12}\ O_{24}$

If the sugar, which may be represented as 12 carbon, 12 hydrogen, and 12 oxygen, can take 18 equivalents of oxygen from the nitric acid, the nitric acid is decomposed, and water and oxalic acid are produced. If I had oxidised the sugar still further, (by burning it with oxide of copper, for instance, in a combustion tube,) what would have been the result? The sugar, with 24 equivalents of oxygen, would have given me water and carbonic acid; thus the sugar, if perfectly oxidised, gives water and carbonic acid; if imperfectly oxidised, water and oxalic acid. To show you that oxalic acid is thus produced, all that is required is to distil off a portion of the nitric acid, to pour the residual liquid into a flat basin, and to leave it until the oxalic acid forms magnificent crystals.

From hence the theory arose, that oxalic acid in the body came from vegetable food. You have seen, that out of the body imperfectly oxidised sugar will produce oxalic acid. Hence, it is possible, that, in the body, imperfect oxidation might be a source, and has been taken as the source, of oxalic acid. It has even been attempted to stop the formation of this oxalic acid, by forbidding all sugar; but, if you remember, in my lecture on the saliva, I showed you how exceedingly difficult it is to prevent sugar from forming even when it is not ready formed in our food. Every particle of starch we eat passes into sugar, and thus not only sugar must be forbidden, but all starch must be abstained from also. A very hopeless proceeding indeed, for it is by no means easy for human beings to live on animal food alone; and, though it can be done, yet to the great majority of us it is nearly impossible. Moreover, it is far from certain that the oxalic acid is derived in the body from the vegetable food, although it was long believed that vegetable food was the only source of oxalic acid; yet now, researches by Professor Liebig have established a totally different theory on the origin of oxalic acid. If I take some

pure uric acid, and mix it with peroxide of lead, suspended in water, and heat them to a temperature of nearly 212°, adding the peroxide as long as I find it undergoes change, that is until I get a black or brownish residue, I find that by thus acting upon uric acid I have certain changes produced. Such a proceeding I have actually performed here in this basin. These substances, uric acid and peroxide of lead, were mixed with water, heated to 212°, and then thrown on a filter, and the filtered liquid passed through; and in it this beautiful crystalline substance which has been formed out of the uric acid, and which is named *allantoin*, is dissolved. Not only have I allantoin in solution, but also I have urea,—a substance which, as you know, occurs in the urine, and which I shall have occasion, in a later lecture, to speak of. Here is a considerable specimen of urea, which has been obtained from the solution. But this is not all the re-action. I find that the peroxide of lead has undergone a change; it is no longer peroxide of lead, but on the filter I have, instead of oxide, oxalate of lead, a combination of oxalic acid and oxide of lead. If I suspend the mass which remains on the filter in a little water, and pass sulphuretted hydrogen through it, sulphuret of lead is produced, and oxalic acid remains in solution I boil it, and set it aside to crystallise,—you see how highly acid the solution is. In this basin you see the crystals of oxalic acid beginning to form. The decomposition of the uric acid is represented in my diagram.

The Conversion of Uric Acid into Oxalic Acid and Urea.

	C	H	O	N	Pb
1 Uric Acid	10	4	6	4	—
2 Peroxide of lead	—	—	4	—	2
3 Water	—	3	3	—	—
	C_{10}	H_7	O_{13}	N_4	Pb_2
Become					
	C	H	O	N	Pb
2 Oxalic acid	4	—	6	—	—
2 Protoxide of lead	—	—	2	—	2
1 Allantoin	4	3	3	2	—
1 Urea	2	4	2	2	—
	C_{10}	H_7	O_{13}	N_4	Pb_2

By adding together 2 equivalents of oxalic acid, 2 equivalents of protoxide of lead, 1 equivalent of allantoin, and one equivalent of urea, precisely the same numbers are obtained by adding together 1 equivalent of uric acid, 2 equivalents of peroxide of lead, and 3 equivalents of water. Thus, then, we have the basis for another theory of the formation of oxalic acid; namely, that it arises from imperfect oxidation of uric acid. When uric acid is further oxidised, as for example, when burnt with oxide of copper or chromate of lead; then carbonic acid, water, and ammonia are produced; but when it is imperfectly oxidised, then allantoin, urea, and oxalic acid are formed. We have thus a very different theory of the production of oxalic acid from that which I before mentioned. Instead of assuming it to be formed from sugar, a vegetable substance, we see that it may be formed from an animal substance,—from uric acid, a substance which never occurs in vegetables, but which always exists in the urine, and has been discovered in healthy blood. I am far from saying that oxalic acid is never formed in the body from sugar; certainly, however, there is no experimental proof that it is formed from vegetable food. On the other hand, it is certain that oxalic acid occurs in the urine when no vegetable food has been taken, and after large doses of uric acid have been taken oxalic acid is said to be found in the urine. The physician also cannot fail to observe that uric acid sediments and oxalate of lime occur constantly in the same kind of cases at the same time; or the deposit, at one period of the day, in the morning, perhaps, may be oxalate of lime, and a few hours earlier or later may be urate of ammonia; thus rapidly alternating, or the alternations may occur at longer periods, as may be seen in some calculi in which layers of urates and oxalates are found frequently to succeed each other many times in the same stone. Hence I have no doubt that these substances are very closely related in their origin; but whether oxalic acid does come from uric acid only, or from some substance closely related *to uric acid, or whether* it sometimes is formed from our vegetable food, cannot,

at present, be determined. At present I consider it is by far the most probable supposition, that the oxalic acid is formed in the body from the uric acid, and not from sugar. I cannot conclude these observations on the occurrence of oxalate of lime without alluding to the influence of lime in determining the formation of oxalic acid in the body; accurate experiments here also are still wanting: but the frequent occurrence of oxalate of lime calculi, and sediments, where the water is very hard, and the advantage derived from soft water, are so striking that they cannot be overlooked.

I must pass on now, for the remainder of my lecture, to a substance which never forms a calculus, but which, nevertheless, is always passing out of the body, can always be detected with the greatest ease, and the study of which alone will enable you fully to comprehend the occurrence of the phosphates in the urine, and the formation of the phosphatic calculi.

All urine contains sulphuric acid, but a calculus consisting of the sulphates is never found. I know of no calculus whatever in which more than slight traces of the sulphates have been detected. The reason of this is, that the alkaline sulphates are very soluble in the urine. The alkaline sulphates do not form concretions because they are not insoluble substances; yet they are always present in the urine, and present in large quantity. Still they do not show themselves by forming sediments or calculi. Just as urea is always present in the urine, but never forms a calculus of urea or a visible sediment, because it is highly soluble in the water of the urine.

If I take a portion of any urine whatever, and accurately fill a 1000-grain bottle, of course, by weighing that bottle, and finding how much it weighs more than when filled with water, I get the specific gravity of its contents. If I take this weighed quantity of urine, the specific gravity of which is thus determined, and add to it a salt of baryta,—muriate of baryta, for instance,—I always get a plentiful precipitate. Now this precipitate, for the most part, consists of sulphate of baryta. To make sure that it consists only of sulphate of baryta, I boil it with hydrochloric acid, which is added in excess; and if there be any phosphate of baryta present, which is soluble in hydrochloric acid, it is taken up immediately, and the sulphate of baryta remains by itself. The liquid is boiled also, in order to produce in the precipitate a more coarse state of aggregation, and thus to enable it to be caught by the filter. Having, then, precipitated the sulphate of baryta, and boiled it for a few moments to make sure that every trace of phosphate of baryta is dissolved, and that the precipitate can be filtered, I throw it on a small filter, and wash it until I find it is thoroughly free from all trace of chloride of barium, or until every trace of soluble matter has passed through the filter. When I am sure that the precipitate is thus thoroughly washed, the filter is burnt in a small weighed crucible, and, on reweighing it, the quantity of sulphate of baryta which is precipitable out of the weighed quantity of urine is determined, and thence the quantity of sulphate of baryta precipitable from 1000 grains of urine can immediately be calculated. By this means I have found, that 1000 grains of urine, in a state of health, of the specific gravity of 1033·9 to 1029·3, will give from 15·23 to 9·49 grs. of sulphate of baryta, when full exercise and full diet are taken by a healthy person; this quantity of sulphate of baryta may be obtained three or four hours after the principal meal. Long after food—that is, after the immediate effect of the last meal has passed away,—I found the quantity of the sulphate of baryta in the urine is diminished; then 1000 grs. of urine, of 1027·6 to 1025·3 sp. gr., will give from 8·56 to 7·07 sulphate of baryta. It follows, that the food distinctly increases the sulphates in the urine. But it is of importance to determine what kind of food produces the most decided effect on the sulphates. Does animal food increase the sulphates to the same amount that vegetable food does? I endeavoured to arrive at the knowledge of the variations produced by different kinds of food by examining the urine in the way just mentioned, when animal food or vegetable food alone was taken. For three days vegetable food alone was taken, and at the end of the third day the quantity of sulphates in the urine was determined. After vegetable food the sulphates are increased—not beyond the average, as you see in this table, but still distinctly increased:—

Effect of Vegetable and Animal Food on the quantity of Sulphates in 1000 grs. of Urine.

	Specific Gravity.	Sulphate of Baryta.
3rd day Vegetable Food.		
Before	1028	9·5 grs.
After	1032	13·7
3rd day Animal Food.		
Before	1025	9·3
After	1026	11·1
After exertion, before food	1031	11·3

So, also, after animal food was taken for three days. On the third day the numbers obtained are represented in the table, and the sulphates are increased after food. I am unable to show you what was the effect of the water which was drunk at each meal; some increase, no doubt, was caused thus, but as the same water and the same quantity of it was taken, whether the diet was animal or vegetable, it does not affect the comparative results. It must not be overlooked, that on the third day, on animal diet, the specific gravity was lower than usual, which partly may account for the diminution in the sulphates, as compared with the amount when vegetable food was taken. I endeavoured to determine the effect of active exercise on the sulphates in urine. I can only point to one analysis which is given in the table above, as an example of the increase of the sulphates after strong exertion before food was taken. You see, that before food, after very violent muscular exertion, every 1000 grains of the urine gave 11 grains of sulphate of baryta; whilst, before food, when much less exercise was taken, there were only 9 grains of sulphate of baryta precipitated from every 1000 grains of urine. Hence, it is most probable, that the influence of exertion upon the production of sulphates in healthy urine is not inconsiderable. And as the same result has been obtained by other observers, there can be little doubt as to the fact. It must not be forgotten, that the diet has a greater effect on the sulphates than the most active exercise. In order to estimate the elimination of the sulphates, it was necessary to trace the effect of different medicines on the sulphates in the urine. In the following table

On the Influence of Sulphuric Acid, Sulphur, and Sulphates, on the amount of Sulphates in the Urine.

	Sp. Gravity	Sulphates
No Sulphuric Acid.	1024	8·2 grs.
½ oz. dilute Acid	1024	" 11·4
Sulphur before Food	1022	" 8·7
" after "	1027	" 14·0
Sulphate of Magnesia	1024	" 22·6

Some of the results are stated for the purpose of comparison. When dilute sulphuric acid of the Pharmacopœia was taken in very large quantity, no very great increase was found in the quantity of sulphates thrown out of the body. When no sulphuric acid was taken, there were 8·2 grains of sulphate precipitated from 1000 grains of urine specific gravity 1024; when half an ounce of sulphuric acid was taken, there were 11·4 grains in the same quantity of urine of the same specific gravity: no very extraordinary increase, considering the quantity of dilute acid which was taken. Without doubt, it does increase the sulphates, but not in a very great degree. If, however, sulphate of magnesia is taken, (for instance, in 2 drachm doses, which is not an inordinate quantity,) I find the sulphates mount up to as much as 22·6 grains in every 1000 grains of urine.

I have already alluded to the effect of sulphur on the sulphates in the urine; and I find, when a continuous course of sulphur is taken, (the diagram represents the average of six days;) that the sulphates, which before the course was begun, were 8·2 grs. per 1000 grs. of urine, now amount, before food, to 8·7 grs. per 1000, and after food to 14 grs. per 1000 grains of urine; that is, the sulphates are increased, but not so much as to go beyond the highest limits which are found in a state of health.

It appears then, that by far the most remarkable results are produced by the sulphates when taken as medicine. The effect of diet is the next most important cause of variation. This last may be the result of the soluble sulphates in the food. The increase of the sulphates in consequence of muscular action, is also important. In my previous lecture on respiration, I mentioned a case in which there was the *effect, not only of intense* muscular motion, but of the medicine also. Then the

greatest increase in the sulphates was to be expected, and was accordingly found. It was a case of *delirium tremens*, in which disease, and in intense chorea, I have especially found the increase in the sulphates to take place. When the patient had taken sulphate of magnesia in two drachm doses, and thus when the disease and the sulphate of magnesia acted together, the quantity of sulphate of baryta precipitable from the urine, amounted to the greatest I have ever found,—as much as 37 grs. in every 1000 grs. of urine. In the same case, when no sulphate of magnesia was taken at all, the quantity was above that of a healthy man on full diet, and in full exercise; it then amounted to 20 grs. in every 1000 grs. of urine.

Now, I have dwelt at some length upon these variations of the sulphates in the human body, in order to show you what may be taking place in the urine, when, to the eye alone, no variation appears to occur. You can never tell, by looking at the urine, nor by taking its specific gravity, whether there is any increase or diminution of the sulphates in urine; you must apply your chemical reagents. When there was the greatest excess of sulphates, there was not the slightest trace of a precipitate in the cases I have mentioned; not the slightest microscopic appearance or form of a crystal, whence it could be conjectured that the sulphates were present in excess; yet the greatest increase may occur, or the greatest diminution may take place, whilst, by mere ocular inspection, you know nothing at all about it. The sulphates in the urine never show themselves; they require to be made to appear; chemical tests are necessary to be certain of their presence. Now, this is the important point as regards my next subject, on the occurrence of the phosphates in the urine. If you will remember, that in the urine substances may exist in increased quantity, or in greatly diminished proportion, whilst the eye can see no difference and can detect no change until chemical reactions cause the variations to become apparent, then I have no doubt you will easily understand the variations of the different kinds of phosphates which form one of the most remarkable excretions from the human body.—*Med. Times, Nov.* 8, 1851, *p.* 477.

67.—ON THE ORIGIN OF OXALIC ACID IN THE ORGANISM.

By Professor Lehmann.

As the use of vegetable food, of which many varieties contain oxalates, increases the quantity of oxalate of lime in the urine, the inference would seem a legitimate one, that the oxalates are transmitted from the food to the urine. The source of this salt must, however, not be sought for only in the preformed oxalates, but in the amount of alkalies in combination with vegetable acids present in the food; for, as we have already mentioned, they induce an augmentation of the oxalate of lime. In all the well-marked cases to which I have alluded, the increase of the oxalate of lime seemed to be combined with disturbance of the respiratory process. Thus it may be easily understood why, after the use of drinks rich in carbonic acid, of alkaline bicarbonates, or vegetable salts, oxalic acid is increased in the urine; the superfluous carbonic acid which has entered the blood, or been generated there from the salts of organic acids, must obstruct the absorption of oxygen and the perfect oxidation of certain substances in the blood: hence also the quantity of oxalate of lime has been found to be increased by the partially impeded exchange of oxygen and carbonic acid in the lungs, consequent on emphysema, pulmonary compression during pregnancy, &c. We might, in such cases, assume, according to a formerly prevalent belief, that the kidneys in some degree acted vicariously for the lungs, since under the form of oxalic acid they remove from the organism the carbon which the latter organs would have excreted as carbonic acid.

Although certain chemists hold a contrary opinion, it is an undoubted fact, that the nervous system has an influence on the oxidation of the blood. The occurrence of oxalate of lime in cases of epileptic convulsions, in convalescent persons, &c., might be referred to the disturbance induced in such cases in the nutrition or in the function of the nervous system, and to its diminished influence on the process of respiration, without there being any necessity for the assumption of a special diathesis.

It seems, moreover, unreasonable to set up such a dinthesis, since the establish-

ment of a special disease from a single symptom—that symptom being only the occurrence of oxalate of lime—is entirely opposed to the spirit of rational medicine.

From Wöhler and Liebig's discovery, that uric acid is decomposed by peroxide of lead into urea, allantoin, and oxalic acid, it has been pretty generally assumed that the oxalic acid of the urine is due to an oxidation of the uric acid; the oxalic acid, in this case, not being converted into carbonic acid, as usually occurs in the healthy organism. That the formation of oxalic acid may be in part thus explained, is unquestionable, but there are many other substances in the animal organism besides uric acid, which, by oxidation, yield oxalic acid. No definite numerical ratio between the uric acid, urea, and oxalate of lime in the urine, has been yet established.

C. Schmidt has pronounced a very ingenious view regarding the origin of oxalate of lime in the urine. He believes that we must seek for the source of its secretion in the mucous membrane of the urinary passages, and that the oxalate of lime is first produced by the decomposing action of the acid urine on a soluble compound, oxalate of albumen lime, secreted by the mucous membranes; for oxalate of lime as an insoluble body could not penetrate with the urine through a series of renal cells; oxalate of lime is also formed from the mucus of the gall-bladder by this mode of decomposition. When oxalate of lime occurs in the urine, we always find an augmentation of the mucus. These reasons do not, however, appear to be so decisive as to induce us to exchange the view we have already given for that of Schmidt; and, indeed, in another place we find Schmidt himself maintaining that the urea is in part combined with oxalic acid.—*Med. Times and Gazette, Jan.* 10, 1852, *p.* 43.

68.—*On the Formation of Lactic Acid in the System.* By Professor Lehmann. —The whole of the chapter on this acid is well deserving of attentive perusal by every one desirous of keeping himself on a level with our present knowledge of physiological chemistry. The following peculiar relation has been recently observed by Engelhardt and Maddrell in the crystallisable lactates. The lactic acid obtained from the animal fluids, and that produced from sugar by fermentation, form with the same base salts which present certain differences in the amount of their water of crystallization, in their degree of solubility, and in their decomposition by heat. This is, however, a subject requiring further investigation, for Liebig thinks that he has obtained from the acid of *sauer kraut* a zinc salt corresponding with that yielded by the muscular juice; and Lehmann, in analysing the lactic acid of the gastric juice in combination with magnesia or zinc, always found it correspond with that obtained from sugar.

We proceed to give a brief extract of the physiological relations of this acid. Lehmann has shown, that in the gastric juice there are both free lactic acid and lactates, in addition to free hydrochloric acid. He has never been able to discover the presence of lactic acid or lactates in the normal saliva either of man or the horse; but in a case of diabetes, in which there was spontaneous ptyalism, he convinced himself, beyond all doubt, of the presence of the free acid. He likewise detected it in the contents both of the small and large intestines of the horse. The recognition of lactates in healthy blood is as difficult as that of urea, and it is probable that we shall never obtain a positive demonstration of the existence of alkaline lactates in the blood by direct experiment, although the simplest induction proves that they must be present there, even if they only remain in it for a short time. Lactic acid may, however, like urea, collect in such large quantities in the blood, as to be capable of detection by chemical analysis. Cases of this sort have been described both by Scherer and Lehmann. It has been detected in exudations after puerperal fever, and in emphysema. Although lactic acid was first discovered by Scheele in milk, it does not occur in the healthy fresh milk of man or animals. After considerable exposure to the atmosphere healthy milk acquires an acid re-action, which is dependent on the formation of lactic acid from the sugar of milk by fermentation. It exists in a free state in the muscular fluid—a fact recognised forty-two years ago by Berzelius, and lately re-discovered by Liebig; and its salts are asserted, by the *first-named chemist, to* occur in the sweat and the bile. Schmidt has separated it

in the form of lactate of zinc from the strongly acid fluid yielded by the long bones in a case of osteo-malacia. The question, whether or not the lactic acid and its salts exist in the urine is one regarding which there has been so warm a contest between Liebig, on the one hand, and Berzelius, Lehmann, &c., on the other, that we shall give our author's present opinions on this point. After very frankly confessing that his earlier analyses were open to deceptions in reference to this acid, he adds, that to maintain that the urine of healthy men and animals never contains lactic acid, or lactates, under any physiological relations, is equally erroneous with his former view, that lactic acid constantly occurs in animal urine, and that the acid reaction of this fluid is solely dependent on its presence.

"A more extended investigation has led me to the following results:—In all cases, where the supply of lactates to the blood is very great,—whether this depends on an excess of acid being formed in the muscles, or on the use of a diet tending to produce it, as on an imperfect process of oxidation in the blood—lactic acid may be detected in the urine with all the certainty which, in the present state of chemistry, can be expected in such researches. Hence we can understand why it is that, in the urine of the same individual, lactic acid may on one day be present, and on another absent; why, in many persons, no lactic acid can be detected in the urine, and in others, again, (and especially in those who, in consequence of repeated catarrhs, suffer from partial relaxation of the pulmonary tissue, and yet often think themselves perfectly well,) it is constantly present in the urine; why stall-fed animals, living on amylaceous fodder, excrete lactic acid by the kidneys, (and in part, also, by the mammary glands,) while under other conditions this acid cannot be discovered in their urine; and why, finally, in most febrile diseases, lactic acid may be recognised in the urine."—*Med. Times and Gazette, Jan.* 10, 1852, *p.* 43.

69.—ON DIABETES.

By Dr. H. Bence Jones, F.R.S.

[It should be borne in mind that the disease under consideration is more especially related to the non-nitrogenous organic constituents of our food. Dr. Jones commences by observing,]

In my lecture on the saliva, I, by especial experiments, pointed out that the action of the saliva on the starch, which we take as food, is similar to the action of a ferment, and causes it to undergo a change into sugar; and I showed how easily this sugar could be found. Since then I have made some experiments on the rapidity with which this conversion occurs; and I find, that if you take a portion of pure starch, and hold it in the mouth for only two minutes, you can obtain distinct and decided traces of sugar. I have here a solution of starch not treated with saliva; and, if I employ my test for sugar, which you well know, (sulphate of copper and liquor potassæ,) I shall have no reduction of the oxide of copper; but, in this other mixture of starch and water, which has been held in the mouth for two minutes only, you may see distinctly a beautiful red line of reduced copper, the evidence of the presence of sugar. If the starch is left in the mouth for three minutes, a still more manifest action is apparent, as in this tube; and, if it remains there five minutes, there is, as you see, a distinct mass of reduced copper, which is proportioned to the quantity of sugar formed out of the starch. In making these experiments, no heat has been employed to hasten the reduction; these mixtures were tested yesterday, left at the temperature of the laboratory during the night, and this morning the reductions were apparent. Thus, then, there are many sources of sugar in the body. It is found, for the most part, in vegetable food already formed; it arises from the action of saliva on starch. It is present in considerable quantity in milk, and minute traces of it are contained in muscle; but still further, you will remember, in my lecture on bile, I showed you that sugar is always produced by the action of the liver. I must refer you to the diagram, which I have exhibited at a former lecture, showing the relation of fat to the bile, indicating that perhaps the fat acid, by the action of the liver, is changed into the bile acid, while the glycerine is changed into sugar and water. Whether this be a true representation or not, it is certain that we have a large quantity of fat going into the liver by the portal vein, and a

large quantity of sugar coming out by the hepatic vein. I pointed out that this sugar is always found in the liver, not only when vegetable food, but even when animal food alone is taken.

Having thus spoken of the sources of sugar in the body, I may now come to that state of disease in which sugar exists in large quantity in the secretion of the kidneys, in which, in health, none should appear. In this respect the secretion differs from milk, which contains sugar when in a state of health, and would be unhealthy if it did not contain it. If the urine contains sugar, it is in a diseased state. In certain cases of disease, a very large quantity of sugar is thus thrown out of the body. It can be separated, weighed, and analysed with considerable exactness. It is found to have the same composition and the same reactions as starch sugar. If I take a portion of this urine, which I have good reason to know contains sugar, and test it with sulphate of copper and liquor potassæ, I get first a beautiful blue solution, and afterwards, by heating it, (the same result would be obtained by letting it stand long enough in the cold,) the change takes place which you have so frequently seen, the blue colour becoming slightly yellow at the surface, and rapidly changing into that reddish yellow precipitate which is the mixture of the colour of the urine, and the colour of the sub-oxide of copper. This is one easy test of the presence of sugar. If this test gives a negative result on repetition, you may be sure that in that specimen of urine no sugar is present. If a positive answer is given, as in the above experiment, some additional test ought always to be employed. For, by the use of the sulphate of copper test alone, very many and great mistakes have been made. With perfectly healthy urine, this test would produce no change.

But it may occasionally happen that urine may be passed which will give a reduction similar to that which I have shown you, which might be confounded, and has to my knowledge been confounded, with the reduction produced by sugar. If I take urine very much loaded with urea, and apply my test, I shall find that the first part of the test (for I may divide it into two parts) will take place,—there will be a formation of the beautiful blue colour. Urine saturated with urea; that is, with organic matter in solution, will give no precipitate of hydrated oxide of copper on the first addition of the liquor potassæ. Urea, like sugar and other organic matter, hinders the hydrated oxide of copper from being precipitated; and thus I have a beautiful blue solution formed. But if I heat this, I shall not get anything like the rapid reduction which you saw take place when sugar was present. It is true I shall get a change by heat, but the reduction will now be slow; and it is on the quickness of the reduction of the oxide of copper that the test for sugar depends. If I take a portion of urine containing an excess of urates as well as urea, I shall not only get the blue colour formed, but I shall have a reduction very much more distinct than with the urea alone, but it will not take place quite so quickly as if sugar was present; it may be so quick, however, that a mistake may be made regarding the presence of sugar. I have here such urine, containing a great excess of the urates. If I boil it after the addition of the sulphate of copper and liquor potassæ, a change of colour will ensue, and a reduction will take place. If urine be examined very soon after it is passed, it often contains so great an excess of urates, that a very considerable reduction will occur on the use of the test. The employment of a second, and in doubtful cases of a third test, can alone save you from saying that sugar is present when it is absent from the urine.

What, then, are those other tests which should be used in addition to the reduction of the oxide of copper? Another metal, instead of the salt of copper, may be used for testing the presence of sugar in the urine. I will take a salt of silver—nitrate of silver—in solution. If I add to a strong solution a drop or two of ammonia, so as to separate the oxide of silver, and then take one drop of diabetic urine, a remarkable change will take place on the application of heat. You will see that the grape sugar will take away the oxygen from the oxide of silver, and the metallic silver will be thrown down, which will adhere to the sides of the vessel, and will produce a most beautiful silvering of the glass. The silvering, indeed, is as perfect as you could produce by the essential oils which are used for this purpose. It only wants a little care and time for a little brisk agitation of the test-tube for the experiment to be successful.

There is another good test for the presence of grape-sugar, and especially for its

presence in urine. If I take a portion of urine containing grape-sugar, and add caustic potash to it, and heat it, a remarkable change will ensue. It will become first brown, and then assume a reddish colour, becoming more and more intense as the heat is continued. We have certain acids produced by the action of the liquor potassæ, more especially melassic acid, which combines with potash, and gives this remarkable colour. But, like all other tests, this has its fallacies, for there are other substances which may be present in the urine, and which may give this darkening of the colour. I showed you that a slight change of colour takes place when pus or albumen and liquor potassæ are heated together. The darkening, however, is not so deep red as it is when sugar is present. You must not trust, then, to this liquor potassæ test alone, but you must use it in conjunction with other tests. A very easy test is the microscopic appearance of a drop of urine placed on a slip of glass and left to dry; when sugar is present the residue presents most remarkable tufts of stellated crystals. A curious instance of the discovery of sugar in the urine was mentioned to me by a medical gentleman of Antigua. He observed that the sugar ants infested the water, and thereby he discovered the complaint.

[Dr. Jones remarks that an apparatus has been constructed by M. Soleil far more convenient, and infinitely more easy of use, than the process which has been noticed, and by it the most perfect results can be obtained. Dr. Jones continues:]

I come now to the nature of the disease, and to the cause of the appearance of sugar in the urine. Whence does this sugar come? This is a problem which is not yet solved. If you were guided by analogy, you would say, as milk sugar is formed by the action of the mammary glands of carnivorous animals, or grape sugar by the liver, so diabetic sugar, in cases of disease, is formed by the action of the kidneys. Whether we shall return to this ancient view of the nature of diabetes is doubtful. At present, the inquiry into the nature of the disease sets out from a different point of view, in which the kidneys are considered as having no action whatever in the production of the disease. The most important arguments in favour of this view will be given by the following statement of the knowledge which we at present possess:—It is quite certain, that if you stop a person who is passing diabetic urine from taking vegetable food, you can materially lessen the quantity of sugar in the urine. If I stop a quantity of sugar from going in, it is certain that I lessen the quantity of sugar coming out. But it must be remembered, that the starch which enters the body is converted into sugar; and thus, if starch be prevented from going into the body, the quantity of sugar in the urine will be still more materially lessened; so much so, that M. Thenard stated that animal diet was as decided a remedy for diabetes as quinine was for ague. There is no doubt that quinine cures intermittent fever; but no abstinence from starch and sugar will cure diabetes. Moreover, no absolute abstinence from starch and sugar will stop all the sugar from appearing in the urine. And why is this? I have already told you, that the liver forms sugar in a state of health; even when no starch or vegetable substance is taken as food, so that by the strictest diet you cannot altogether stop its appearance in the urine. You can lessen it, but not entirely stop it, so long as the action of the liver continues. The liver, when no vegetable food is taken, continues its action, changing some principles of the food into sugar, which afterwards can be found in the urine. Moreover, in the muscles there are minute traces of sugar; so that if a patient is kept upon animal food alone, some small portion of sugar is taken even when animal food only is eaten. We are led, then, to the following statement of the nature of diabetes. The disease consists in an arrest of those changes in the sugar which should take place in the body in a healthy state. In health the sugar undergoes changes into other substances; it does not remain as sugar in the circulating blood, and therefore no sugar ever passes off in healthy urine. I say that in this disease, the sugar, from whatever source it comes into the blood, does not undergo its proper changes; it goes into the blood, remains there as sugar, and is thrown out as such in the urine. Now what are the changes which occur in a state of health? The starch going in is changed into sugar; the sugar is changed into organic acid, as lactic acid, and ultimately it is changed into carbonic acid and water, passing out by the lungs. But not only is lactic acid produced; many other acids, as acetic acid, butyric acid, formic acid, and even valerianic acid may all arise from the changes which the sugar undergoes. We do not know why the sugar undergoes these changes in health.

The changes are not yet fully known, much less the causes of the changes. It is in the highest degree probable that the alkaline state of the blood promotes the changes, just as the alkali in the test for sugar with sulphate of copper caused the oxidation of the sugar which took place. I say there is little doubt that the alkali in the blood has, in health, a decided action in causing these starchy and saccharine substances to be thus changed. See, then, the relation of these changes to diabetes. In diabetes we have the starch going in, and being changed into sugar, and into nothing further.

Changes in Non-nitrogenous Organic Substances.

In Health.	In Excessive Acidity.	In Diabetes.
Starch	Starch	Starch
Sugar	Sugar	Sugar
Lactic acid	Acid	—
Carbonic acid	—	—

This diagram shows the changes which take place in non-nitrogenous organic substances in a state of health: the starch undergoes a change into sugar, lactic acid, and carbonic acid. But there is another state which I have mentioned in a previous lecture,—that of excessive acidity,—in which the changes do not go so far as they do in health; the changes are starch, sugar, and organic acid, not going so far as carbonic acid, but producing only lactic acid, acetic acid, formic acid, and perhaps oxalic acid, &c. I have already spoken of urine possessing an intensely acid character. I have here urine which was passed some months since, but which has never yet gone on to putrefaction, but remains as intensely acid as it was the first day it was passed. This intense acidity appears to be the intermediate state between a state of health and a state of diabetes. I have watched these cases of excessive acidity, to try and find whether occasionally I could not discover sugar to be present; but as yet I have not been successful, though there is no doubt that a state of extreme acidity not unfrequently goes before the diabetic state. But I cannot find that diabetes very often alternates with a state of excessive acidity. As yet I have not met with many well-marked cases of this alternation. However, in the treatment of cases of excessive acidity, I find the same difficulty as in cases of diabetes,—a difficulty in causing the sugar or the acid to change into carbonic acid and water. I want some means of making the sugar and acid undergo these changes. In two or three cases of excessive acidity, I have given alkalies, and kept the urine alkaline for weeks; but, as soon as the treatment was left off, in two days the urine has deposited uric acid in considerable quantity. Even when a patient has been kept on this treatment for months, I have had a considerable quantity of uric acid thrown down two days after the treatment had ceased. Occasionally excessive acidity seems to me to be nearly as difficult to cure perfectly as diabetes; and that there is the closest relation between the two states, I hope to make more evident at some future time, but I have not obtained the full proof of their relation now. Nor can I now explain the reason why, in diabetes, the changes of the sugar do not take place as in health. M. Mialhe considers that the sole cause of the disease is the absence of sufficient alkali from the blood. Others think that the disease arises from faulty action of the liver. I have already stated, that the liver is undoubtedly the source of one portion of the sugar; but I cannot consider that diabetes is caused by excessive formation of sugar in the liver. The cause of the arrest of the healthy changes of the non-nitrogenous organic substances has yet to be discovered.

There is no doubt that alkalies have formed the most beneficial part of the treatment of diabetes. Alkalies, in considerable quantity, have as yet proved more beneficial than any other mode of treatment; at the same time, substances should be given which cannot form sugar, and especially fatty substances, which can be used in the process of respiration. The patient may thus even gain in weight, and the disease be materially checked. In slight cases, occurring in very fat patients, especially where the disease has manifestly some relation to gout, alkalies are far more effectual than in the more severe and confirmed cases in which there is great emaciation.

We may then sum up shortly thus. Sugar, we have seen, may come, and does come into the blood from vegetable and animal food. Each moment in health the blood is *forming sugar*. Whencesoever the sugar may come in health, it is changed

into organic acid, and ultimately into carbonic acid and water. In diabetes, from some unknown cause, the sugar in the blood is not changed; it is thrown out as sugar. We may stop all the starch and sugar from going in as food, and thereby lessen the quantity of sugar thrown out in the urine, but there still remains the sugar which is formed by the liver; this is unchanged, and still passes off in the urine. The nature of the disease will be fully understood when we know why it is that the sugar which comes from the food and from the liver does not undergo those changes which it does undergo in the state of health; and when we can find the means which will cause these changes to take place, then the certain cure for diabetes will be found. The discovery by M. Bernard, of diabetic urine in the fœtal state, in my judgment, points to the want of oxidation as the cause of this disease; and as gout partly is caused by a want of oxidation of uric acid, the representative of the albuminous principle, or the nitrogenous substance, which is passing out of the body, so perhaps diabetes may be taken to result from an arrest of the action of oxygen on the non-nitrogenous constituent of our food.—*Med. Times and Gazette, Jan.* 31, 1852, *p.* 101.

70.—ON THE TREATMENT OF DIABETES.

By Jabez Hogg, Esq., London.

[From a careful review of the pathology of diabetes, Mr. Hogg was led to the following considerations as to the treatment.]

The chief indications of treatment appear to be the prevention of the elaboration of the saccharine poison in (the *laboratory of life*) the stomach, and to restore the defective power of the digestive apparatus. These ends I sought to fulfil by employing some medicine that would effectually arrest the saccharine fermentation; and I attempted to accomplish my object with a sulphuret of arsenic, administered as I shall detail in the following case which came under my charge.

The chemical difficulty that presented itself to me was the conversion of the sulphuret of arsenic into a *soluble sulphuret*, which would not accumulate in the system, and become a more than ordinarily dangerous remedy to deal with. This I was enabled to effect by combining the hydrosulphuret of ammonia with the liquor arsenicalis.

The first trial I made of this preparation was upon a poor man, J. G., living near Burton-crescent, in a cellar, and otherwise, too, most unfavourably situated for a trial of the remedy. When he applied to me he had suffered about two years from the disease; he had been an inmate of two hospitals, and was discharged unrelieved from the second one about six weeks previous to my seeing him, which was on the 14th December, 1850. He had then been entirely confined to his bed for twelve days; during the last twenty-four hours, and for many days previous, he had passed about three gallons of urine, very sweet to the taste, and of the specific gravity 1·050. Trommer's test showed the presence of sugar, and upon the addition of *yeast* I obtained several grains of sugar, the crystals of which I satisfactorily determined under the microscope; pulse small and quick, 120; cough very troublesome; face œdematous. Ordered a meat diet, with a little toasted bread; and as the thirst was great, tea only, without milk or sugar, with a tablespoonful of the following mixture every four hours:—Liquor arsenicalis, two drachms; hydrosulphuret of ammonia, twenty minims; tincture of henbane, and tincture of lavender, each two drachms; infusion of buchu, eight ounces. To have five grains of soap-and-opium pill at bed time.

Dec. 16th. A decided improvement manifest; had passed only his usual quantity of water; specific gravity, 1·045; slept several hours; cough and œdema relieved. Continue mixture and pill at night.

11th. Improving; pulse 100; not so irritable; urine still decreasing in quantity; specific gravity as before; appetite more natural; cough not so troublesome; had had some comfortable sleep. To continue the medicine.

20th. Three quarts of urine had been passed during the last twenty-four hours; specific gravity 1·040; cough troublesome, from having neglected to take his pill; bowels confined. Continue mixture, omiting buchu, and adding

infusion of calumba. To have ten grains of aloes-and-myrrh pill at bed-time.

22nd. Decidedly better in every particular. To continue medicine.

26th. Improved in appearance, and able to sit up nearly all day; urine, specific gravity 1·035; two quarts only passed during twenty-four hours; cough not so troublesome, and expresses himself greatly relieved. To continue the same plan of treatment.

Jan. 2nd, 1851. Had been living too freely during Christmas; his cough more troublesome, and general health disturbed; but the urine was not affected, nor increased in quantity. To continue the mixture and take two soap-and-opium pills during the next twenty-four hours.

6th. He was again improving, and I ordered him an additional senna draught to act a little more freely upon the bowels, and to continue his mixture.

10th. He was in every respect doing well.

16th. The urine up to this time had been passed in natural quantities; specific gravity 1·025, and I could scarcely satisfy myself of the presence of sugar. His bowels continuing costive, I discontinued the mixture, and ordered him a grain of quinine, with small doses of sulphate of magnesia in infusion of roses three times a day, with the soap pill as before; and up to the 30th he continued to improve; the cough only remaining to give me any anxiety, he having suffered from a winter cough for many years. I advised him to remove from his underground dwelling to one lighter and more airy. I now felt assured that I had completely arrested the diabetic disease, and saw no more of my patient for many weeks, when I was again sent for, and found him in this same miserable underground lodging. Thus unfavourably circumstanced, and badly fed, phthisical symptoms set in, and of this disease he died at the end of three months; but without the diabetic state of the urine having reappeared, as I carefully noted its condition from time to time. I regret being unable to supply the post-mortem appearances, the friends most obstinately refusing me permission to examine the body.

I have during the year treated another case in a much earlier stage of the disease, and consequently of a milder form, and less complicated than the one above given, with smaller doses of the same mixture, and was gratified to observe the same rapid change. The patient, a boy aged sixteen, was so far recovered at the expiration of a fortnight, as to be enabled to remove to the sea-side: he has had no return of the disease, and is now in perfect health.—*Lancet, Jan.* 3, 1852, *p.* 6.

71.—*New Test for Sugar.*—It is stated by Professor Böttcher, that the least quantity of sugar in urine, or any other fluid, may be detected by adding a little carbonate of soda and a small quantity of magisterium bismuthi, and boiling briskly; when the liquid cools, the bismuth, if sugar be present, is reduced, and forms a black powder.—*Deutsche Klinik.—Med. Times and Gazette, April* 10, 1852, *p.* 374.

72.—*On the Mode of Testing for Glucose or Diabetic Sugar.*—The only tests on which Lehmann places any reliance are Trommer's, the fermentation test and the developement of the torula, and the application of Biot and Soleil's polarising apparatus. The following is the best method of applying Trommer's test to an animal fluid suspected of containing sugar.

"The fluid to be examined is treated with caustic potash and filtered if necessary,—that is to say, if there be too great a precipitate; an excess of caustic potash is productive of no harm, as it should be present in more than sufficient quantity to decompose the sulphate of copper; the latter, which must be added gradually, and in a diluted state, usually gives rise to a precipitate, which disappears when the fluid is stirred; as the quantity of the oxide of copper which is soluble is proportional to the quantity of sugar which is present, very little sulphate of copper must be added at a time, if we suspect that only a little sugar is present in the fluid. On allowing the azure solution thus obtained to stand for some time, there is usually formed a more pure red or yellow powder than the precipitate which is

at once thrown down on boiling the fluid. Moreover, very prolonged heating is improper, for there are several substances which by prolonged boiling separate suboxide of copper from alkaline solutions of oxide of copper; amongst them we may especially name the albuminous substances, which with oxide of copper and potash yield very beautiful azure-blue, or somewhat violet solutions, and by very prolonged boiling, separate a little suboxide of copper, although without the aid of heat they have not this property.

"If a specimen of urine contain very little sugar, or if we are searching for sugar in some other fluid, it is advisable to extract the solid residue with alcohol, to dissolve the alcoholic extract in water, and to apply the potash and sulphate of copper to this solution. By proceeding in this manner, we usually obtain the re-action in its most distinct manner. If, however, we are seeking for very small quantities of sugar, as for instance, in chyle, blood, or in the egg, we must neutralize the aqueous fluid, previously to its evaporation, with dilute acetic acid, in consequence of the solubility of albuminate of soda or of casein in alcohol, thus preventing any albuminous body from remaining in solution. If the re-action do not properly manifest itself in the alcoholic extract thus obtained, or if we would carry the investigation further, we must precipitate the sugar from the alcoholic solution by an alcoholic solution of potash, dissolve the compound of sugar and potash in water, and now apply the sulphate of copper: if only a trace of sugar be present, we obtain a most distinct and beautiful reaction.

[This excellent notice of Lehmann's work in the 'Medical Times and Gazette' is ended by a well-deserved compliment to Dr. G. E. Day, the translator, in the following words, with which we cordially agree:]

We cannot conclude this rather long review of Lehmann's really great work without expressing our conviction, that the Council of the Cavendish Society have shown great judgment in selecting the present work, and intrusting its translation to so able a chemist and scholar as the Editor of Simon—the talented Professor of Medicine in the University of St. Andrews.—*Med. Times and Gazette, Jan.* 17, 1852, *p.* 69.

73.—ON THE EMPLOYMENT OF UREA AS A DIURETIC.

By Dr. T. H. Tanner, London.

[The operation of diuretics is well known to be remarkably uncertain, and in no cases more so than for removal of dropsical effusions. This was the cause of Dr. Tanner's employing urea as a diuretic, forming, as it does, nearly half the solid constituents of the urine. Dr. Tanner observes]

Urea was first obtained pure by Dr. Prout, in 1817; its artificial production from cyanic acid and ammonia was discovered by Wöhler, and is interesting as being the first example of an organic product artificially formed. Dr. Prout thus describes the method of procuring it. "Fresh urine is to be carefully evaporated to the consistence of a syrup, and to this, when quite cold, pure concentrated nitric acid is to be added by degrees, till the whole becomes a dark-coloured crystallised mass, which is to be slightly washed with cold water, and suffered to drain. To this mass is then to be slowly added a pretty strong solution of the subcarbonate of potash or soda, till the whole becomes neutral; and the solution thus formed is to be carefully concentrated by evaporation, and set aside, in order that the nitre formed may crystallise, and thus be separated. To the impure solution of urea left, animal charcoal is to be added in such quantity as to absorb the whole fluid and form a thin paste, which may be suffered to lie for a few hours. Cold water is to be added to this paste, which separates the urea, and the colourless solution thus obtained is to be slowly evaporated to dryness at a low temperature. The resulting mass is then to be boiled in strong alcohol, which takes up the urea, and leaves the remainder of the nitre and most of the other saline substances behind; and from this state of solution in alcohol the urea may be readily obtained crystallised and pure, though it is generally necessary to repeat the process of crystallisation from the alcohol two or three times."—Medico-Chirurgical Transactions, vol. viii., p. 528.

Another plan has been proposed by Dr. Gregory for obtaining this agent, more

ding to which the concentrated urine is to be saturated by oxalic acid, crystals of oxalate of urea being produced; these, dissolved in water, decoloured by animal charcoal, and decomposed by digestion with carbonate of lime, yield a solution from which colourless crystals of urea may be procured.

The easiest and cheapest way of preparing it, however, is found to be artificially from cyanate of ammonia, by the process recommended by Liebig. Twenty-eight parts of dried ferrocyanide of potassium and fourteen of peroxide of manganese are mixed in powder, and calcined upon an iron plate, heated to dull redness; the mixture takes fire, but it is gradually extinguished, and must be stirred while cooling, to prevent agglutination. The cold mass is then powdered, and digested in cold water which dissolves the cyanate of potash; this solution is filtered off, and set aside; the remaining powder is then washed with a second portion of cold water, and again filtered, and in this liquid is now dissolved 20·5 parts of sulphate of ammonia, and the solution added to the first filtered solution of the cyanate. A large quantity of sulphate of potash is deposited, which is strained off, and the filtered liquor now containing, with some sulphate of potash, all the cyanate of ammonia, is evaporated to dryness, during which process the cyanate of ammonia is transformed into urea. The dry mass is digested in alcohol, which only dissolves the urea, and yields it pure on evaporation.—Liebig, Annalen der Pharm., xxxviii. 108.

In whatever way urea is procured, it appears in the form of delicate silvery acicular crystals, which are seen by the microscope as four-sided prisms; they are colourless when pure, free from odour, and generally resembling nitre in appearance, possessing also a similar cooling saline taste. The crystals are soluble in their own weight of cold water, and in every proportion of hot, their solution being neither acid nor alkaline. The composition of urea is C_2 O_2 H_4 N_2.

The preparation which I first employed was obtained by Mr. Garden, of Oxford-street, from urine; Mr. Cocksedge, chemist, of New Oxford-street, has since procured some from the same source, and has prepared some of the prescriptions in which I have ordered it. It is, of course, unnecessary to say, that whether this agent be procured from urine, or prepared artificially, it ought to possess the same properties, and consequently, be capable of producing the same effects. I might, however, remark, that I prefer the preparation obtained from the urine, my reason for this preference being, that in trying some experiments upon a dog, I found that the urea obtained from the cyanate of ammonia was less efficacious. At the same time, I would mention, that as my experiments were very roughly carried out, the difference may have been due to other unobserved causes. It was unnecessary to resort to experiment to prove that urea, medicinally administered, does not exert any deleterious or poisonous agency upon the system, because this had already been done by Dr. Todd. This gentleman injected half a drachm of urea into the vein of a dog, the only effect produced being an excessive secretion of urine, so that the place where the animal was kept was literally flooded in an hour or two by the frequency and quantity of his micturition. In the human subject a copious diuresis has been the only effect produced; in none of the cases in which I have employed it has it given rise to any unpleasant symptoms whatever. The ordinary dose, on its first being used, is ten grains every six hours, dissolved in water flavoured with syrup; as its effects decrease, the dose may be augmented to a scruple or more. At the same time, its action should be aided, as that of all diuretics should be, by the free administration of diluents, as well as by keeping the skin moderately cool. From observation of the mode in which medicines are prescribed in the present day, I fear that by far too little attention is paid to those collateral circumstances which assist the action of all remedies, and which in certain cases, if unattended to, render our efforts useless, effects being produced the opposite to those intended. Boerhaave, the great medical luminary of the 17th century, first pointed out that many diuretics may be so administered as to prove sudorifics; and Dr. William Alexander taught that "the salt of tartar and of nitre, though among the most powerful diuretics, when taken with large quantities of warm liquids, if the body be well covered, prove excellent sudorifics, and do not increase the quantity of urine."—Experimental Essays: London, 1770, p. 155.

None deny the truth of these observations: I would only intimate that they are *not* sufficiently attended to.

It would be worse than useless to occupy the space of this journal by a recital

of the few cases in which I have employed this agent as a diuretic. It may suffice to say, that, in the first instance, and the one in which I more particularly noted its effects, the quantity of urine secreted in the twenty-four hours previous to its administration was only fourteen ounces (high-coloured, acid, sp. g. 1018), whereas, in the succeeding twenty-four hours, during which three doses of ten grains each were administered, the secretion amounted to forty-four ounces (pale, acid, sp. g. 1013). The remedy was continued for the ensuing nine days, in doses of ten grains every six hours, during which period the urine varied in quantity from forty-nine to thirty-eight ounces. At the end of this time it was discontinued, as the dropsy had been temporarily removed; and, on again having recourse to it three weeks subsequently, its effects were as satisfactory.

In conclusion, I would remark, that urea has been prescribed before, especially by some French physicians. The object of these remarks, therefore, has been, not to introduce a new remedy, but to bring forward one which does not appear to have been sufficiently appreciated.—*Med. Times and Gazette, May* 8, 1852, *p.* 464.

74.—ON THE MORBID CONDITIONS OF THE KIDNEY, GIVING RISE TO ALBUMINURIA.

By Dr. C. Handfield Jones, F.R.S., &c.

It has been well pointed out by Mr. Bowman, how peculiarly the vascular system of the organ is disposed; how the short and wide renal artery suddenly breaks up into small straight vessels, going direct into the Malpighian tufts; how the stream of blood thus arriving in those curious capillary loops, is thence checked and stayed, not having an ample, but, on the contrary, a rather narrow channel of egress by the efferent vessel, which passes into the tubular plexus. The effect of this arrangement is, of course, to bring a more than ordinary pressure to bear on the membrane of the capillaries: this, even in their healthiest state, must be greater than that exerted by the blood on any other capillaries; but if congestion arises, or the engorgement stage of inflammation, this strain will doubtless be yet further and in a greater degree increased. The membranous wall of these capillaries (I am of course speaking of those of the Malpighian tufts) is strong and well marked; and it needs so to be, for these vessels, unlike any other that exist in the body, except perhaps those of the choroid plexus of the cerebral ventricles, are bare, not imbedded in the tissue they supply, and not supported by fibre or solid substance. How differently circumstanced are the capillaries of the liver; and how much rarer is hemorrhagic effusion in this organ than in the kidney! Everything indicates that the natural and healthy function of the Malpighian capillaries is to effuse fluid; but fluid of what kind? Exact proof is perhaps impossible; and opinions are much divided. Mr. Bowman believes that water only is poured out from the Malpighian tufts; Mr. Simon, that a solution of albumen is effused; Ludwig, that the fluid is urine containing a large proportion of water, much of which is afterwards absorbed by the tubular venous plexus. Valentin holds the converse opinion, that the fluid from the tufts is at first very concentrated, and afterwards becomes diluted. I feel much inclined to adopt Mr. Bowman's view, because analogy makes it probable that the epithelium of the tubes is the agent in conducting the secretion of the solid matters; and experiments have shown that animal membranes are capable of altering the composition of fluids that pass through them.

[The condition of the kidney—the consequence of acute albuminuria, is not often found after death. When an opportunity is presented of inspecting a kidney in this state the size and weight are found greatly increased, even to twice its normal proportions, from the intensely congested state of its structures. When the congestion arrives at a certain point, extravasation takes place, and blood is poured out from the Malpighian capillaries, and from thence along the tubes.]

The epithelial lining of the tubes is not apparently much altered; its function is doubtless more or less interfered with, partly in consequence of the congestion of the vessels, and partly from the plugging up of many of the tubules with coagulated fibrine. This substance I believe to be effused from the Malpighian capillaries in a fluid state, as liquor sanguinis more or less modified, to pass down the tubes

for a variable distance, and to coagulate there, either by itself alone, or mingled with various forms of epithelial matter, or together with these and blood globules. For a certain time, which varies according to circumstances, the fibrinous coagula remain where they have formed in the tubes; but, as the fluid accumulates behind them, they are forced on down the canal, and at last are swept out, and appear in the urine, whence they form, together with other matters, the well known red-brown sediment, so characteristic of acute albuminuria. The colour of the sediment is of course derived from the blood, the colouring matter of the globules exudes from them, and tinges the epithelial particles and the fibrinous casts themselves.

[Dr. Jones does not see why an engorged kidney should not be as completely restored as an engorged lung, nor does he think the removal of the exudation matter more difficult in the one than the other.]

The next anatomical condition which I propose to describe, is that of the large, more or less hard, and sometimes mottled kidney. This is considered by Frerichs to be the sequel of the first condition, which we have just considered; and in this opinion he is no doubt supported by others. Rokitansky, in particular, regards the mottled kidney as having its origin in a preceding hyperæmia. This point will be more properly considered when we have described the structural changes. The kidney is enlarged, its size and weight increased, even beyond that which occurs in the acute attack; it may rise as high as twelve or fifteen ounces. Its colour is pale, grayish, or white, mottled or marbled, in various degrees, with streaks or patches of vascular redness; this applies to the cortical structure, the medullary usually presenting a contrast by its uniform and deep congestion. The capsule sometimes adheres closely, more often is easily separable from the surface. The tubes of the medullary cones seem to be separated from each other towards the base, and by their divergence produce an appearance not unlike a plume of feathers, or a sheaf of corn. On making a section of the cortical substance, the cut surface presents a peculiar confused aspect, not easy to describe, but such as one might conceive would be produced by a quantity of rather opaque matter coagulated in the interstices of a regular tissue. The surface is smooth, and presents nothing of a granular aspect. On microscopic examination, the cortical tubes are found more or less filled with altered epithelium; sometimes the quantity is so great as to obstruct and block up the tube; more often, I think, the canal is not entirely obstructed, but narrowed. Sometimes the epithelial particles are very perfectly formed and distinct; sometimes they seem to be lost in a dense stratum of granular matter. Often I have noticed that the epithelium has a peculiar *stiff* aspect, such as, compared with the natural condition, gives the idea of its vital actions or changes being arrested in part, or less readily carried on. A considerable quantity of oily matter in the form of minute drops is not unfrequently present in the epithelium; and when it is abundant, the kidneys have a decidedly milky white aspect. This oily matter, I think, is most often present in the medullary tubes near the base of the cones; it seems to be washed down from the cortical tubes, and to collect in this situation. In the interior of the tubes, there are often seen fibrinous casts, and sometimes globules of a yellow matter resembling very much those so frequent in the spleen, and which, like them, are probably derived from altered blood globules, or from effused hæmatin. The basement membrane of the tubes is often in great part lost; the tubes still retain perfectly their contour; but it is impossible to isolate a tube with its investing homogeneous sheath. This I think a very important circumstance, and one which seems to indicate clearly the influence of a degenerating process.

The Malpighian tufts are variously altered; some remain healthy, others are covered with films of coagulated fibrine, which obscure the capillary loops; sometimes the quantity of exudation is so great in the capsule as to compress the tuft, and reduce it into a small compass. The urine is mostly pale; contains a light-coloured sediment, consisting of casts, renal epithelium, and blood globules, in varying proportion. The foregoing description I wish to be very general, and to apply to all instances of enlarged kidney not decidedly hyperæmic, of confused or dense texture, and presenting under the microscope a more or less considerable alteration and increase of the epithelium lining the cortical tubes, without any *actual destruction of* them. Such a condition, I believe, may often ensue as the

result of an acute attack of engorgement, which has been imperfectly subdued; but my impression is, certainly, that more often it is preceded by no decided hyperhæmic stage, but that it arises insidiously, and proceeds without any noticeable disturbance of the system, until at last some secondary disease, the result of the unhealthy state induced in the blood, calls attention to itself, and perhaps to its latent cause. The fact to which Mr. Simon refers in his most instructive paper, in the 'Medico-Chirurgical Transactions,' on subacute inflammation of the kidney, viz., that the mottled kidney is essentially scrofulous, confirms the view I have taken. Many of the cases I would include in the category just described, doubtless owe their origin to such an alteration of nutrition as may be properly termed subacute inflammation. In others, and I think the majority, the process is more purely and simply degenerative; while probably in all, at some period of their course, one form or other of morbid change predominates. This condition of kidney is very much more frequently found than that of acute engorgement; the number of instances observed by Frerichs amounted to a hundred and thirty-nine out of two hundred and ninety-two.

The last general condition of the kidney which I propose to consider, is one which is perfectly familiar to us all, and which, from my own observation, I think is certainly the most common; that in which the organ is dwindled, contracted and granular on the surface. This Frerichs considers as the third stage in his arrangement; he calls it the stage of retrocession, of atrophy. The kidney, far from being enlarged, is in many cases exceedingly diminished, both in size and weight, sometimes descending so low as one and a half or two ounces. On the other hand, its consistence is greatly increased; it has often a leathery toughness, which is the more marked in the same ratio as the atrophy. The colour of the surface is more nearly that of the natural state than it was in the preceding condition; and this appears to depend on the presence of a greater quantity of blood. The capsule is always very adherent to the surface, sometimes scarcely separable without laceration: it is not unfrequently thickened in radiated patches, and probably owing to this thickening may be split sometimes into two layers, a circumstance which I have seen to prove a cause of error, by leading to the opinion that the capsule, after its removal, had left a smooth surface, when in reality its deepest layer still remained adherent. The external form is often decidedly lobulated, reminding one in this retroceding condition of the fœtal kidney advancing in development. The granulations or prominences strewed over the surface are of a lighter colour than the intermediate tissue, and sometimes contrast with it very strongly; their size varies from that of a pin's head or poppy-seed, which is most common, to that of a hemp-seed or small pea. The wasting of the organ is especially manifested in the cortical structure, which is often reduced to a mere stratum a few lines in diameter; it also presents granulations in its deeper parts, similar to those on the surface. Wasting of the medullary cones also occurs, but to a less degree.

On microscopic examination of the cortical tubuli, nothing can be more marked than the utter atrophy by which they have been affected. In extreme cases, one may scarcely find in a section anything except mere granular *debris*, some of these perhaps still preserving the contour of the original tube, but the greater part constituting an indefinite shapeless mass. In other less advanced cases, and in some parts indeed of all, the tubes are still discernible: they are irregularly distended and opaque, with granular contents, which have well-nigh, or perhaps completely, blocked-up their canals. Oily molecules, sometimes accumulated in considerable quantity, lie here and there among the granular matter, and increase the opacity greatly. The granulations are made up of the infarcted convolutions of tubes, and seem to be the parts in which those traces of the natural structure still persist; they remain prominent, because the intervening parts, which are atrophied in a greater degree, have collapsed and shrunk in. Traces of basement membrane, according to my own observation, and that of my colleague, Dr. Sieveking, are generally indiscernible; it appears to perish and disappear, while yet the outlines of the tubes are tolerably preserved. In this respect, I differ entirely from Frerichs, who states that he still finds in the atrophied tissue the remains of the homogeneous membrane. This author also has found occasionally, in atrophied kidneys, new-formed fibrous tissue; he allows, however, that it is but rarely present, and that its production is by no means the essential element of the disease. My observation

quite accords with that of Mr. Simon, that the generation of new fibrous tissue in the kidney is somewhat doubtful, and that it has not much to do in occasioning the pervading atrophy. The Malpighian tufts, in consequence of the general collapse, appear closer together; a few of them remain tolerably healthy, others are compressed and shrunken: often the capsule is filled to a greater or less extent with an oily-looking material. I have not always been able to distinguish satisfactorily between the Malpighian bodies in this state, when their capillaries are greatly compressed, and the capsule partially filled with oily contents, and cysts which are so commonly found in kidneys thus affected. The tubuli of the medullary cones appear generally more affected by infarction than by atrophy; they contain an altered, more coarsely granular, and hypertrophied epithelium, often mingled with considerable quantities of oily matter. I am inclined to think, that as the cortical structure, the normal seat of the secreting process, perishes, its function is in part taken up by the basal part of the medullary cones which adjoin it; the medullary portion, at any rate, suffers far less from the atrophy than the cortical.

Thus far we have traced the atrophic changes of the original structure; but we have now to consider those peculiar formations which, at least by their form and number, appear to indicate the agency of an imperfect *vis reparatrix*, unable indeed to produce aught that can efficiently compensate for that which has perished, or which is capable of any higher development, but still, by its resemblance in exterior form to other natural parts, forcibly reminding the inquirer of that wonderful reparative power which the healthy organism so often displays. The cyst formation to which I allude, was, I believe, first correctly viewed and fully described by Mr. Simon, in his paper on subacute inflammation of the kidney. They had been often noticed before, and various opinions entertained of their origin, such as that they originated from dilatation of the Malpighian capsules, or from distension of the urinary tubules; but no one, I believe, had shown in what great numbers they often existed, how minute was their original size, and how much of the apparent magnitude of a kidney might depend upon their presence. Since I first read his description, I have been in the constant habit of examining these curious formations; and, while well aware that other opinions respecting their origin were more generally held, I have never met with any evidence derived from my own observation that really militated against his view, but with much that tended to support it. Mr. Simon describes the cyst formation as a manifestation of new structure, as independent and superadded, though lowly organised growth. He believes each cyst to have its origin in a germ or nucleus particle, such as under healthy conditions might have produced an epithelial cell. This germ, instead of forming a nucleated cell, developes itself into a vesicle with clean sharp outlines, indicating the existence of a strong homogeneous envelope. The cavity is occupied sometimes by a clear fluid, sometimes by a granular matter, or by various admixtures of granular and oily matter. The size of the cyst varies extremely; the most minute are not larger than an epithelial particle, say 1-1000 inch, the largest may attain the magnitude of a cocoa-nut. This minuteness of the younger cysts seems to me, as it did to Mr. Simon, a conclusive argument in favour of the view above taken of their origin. If, as Drs. Johnson and Frerichs believe, they are dilated portions of tubes, how could they ever be seen of a magnitude inferior to that of the tubes themselves? The envelope of these cysts is occasionally of some thickness, appearing sometimes laminated, sometimes as if a second cyst were included within the first. It is rare that they possess a nucleus or any epithelial formation in their interior, but this I have certainly seen once; it seems to indicate a somewhat higher grade of development. The cysts are generally found in greatest numbers in the superficial structure of the cortical substance; and it is here that they attain, I think, their largest size, in the *same situation precisely* (be it observed) in which the degeneration of the natural structure attains its maximum. This, I think, is not destitute of significant import, in the view of their quasi-compensatory nature. Occasionally, however, they affect another situation, namely, at the bases of the medullary cones, just where these spread out into the cortex: when they are numerous in this part, I think they are generally absent from the surface. They are almost invariably closely impacted in the surrounding tissue, *and are difficult to isolate* completely; a circumstance much relied on by those

who consider the cysts to be formed by portions of a tube being cut off by adhesion of its walls above and below a point, while the included part is distended into a pouch. That some cysts may not form in this way I would not for a moment maintain; I think it quite possible that they may, and also from the Malpighian capsules, when the tube below is obliterated; but I do not think this is the mode of origin of the multitudinous growth which crowds so closely, in some specimens, the field of the microscope. One argument, which weighs very strongly with me in favour of Mr. Simon's view, is the fact that they are by no means found in all atrophied kidneys. Degeneration may have advanced to an extreme degree, without the production of any, or but a very few, cysts: on the other hand, numerous cysts may exist, without the tubes being in any great degree broken up: thus some other cause than the change taking place in the tubes seems requisite for the generation of cysts. The contents of a large cyst, which I recently examined, was a yellowish turbid fluid, abounding with large and small yellow-tinged granular corpuscles; in another, there were the same, with numerous free nuclei. A beautiful epithelial pavement, consisting of closely apposed nuclei, seems to have constituted a lining to the larger. Dr. Bence Jones's researches into the chemical composition of the contents of these cysts, have shown that they do not contain any of the organic principles of the urine, and thus that they are not, *quoad* function, compensatory structures. They contain aqueous and oily matter, and certain modifications of albumen. Mr. Simon, in two instances, detected a large proportion of xanthic oxide in them.

[What is the essential nature of the morbid change by which a healthy kidney is reduced to this condition? Dr. Prout, with an intuitive perception of truth, saw clearly that the morbid process in Bright's disease was different from that belonging to any variety and type of inflammation. He called it, and justly so, a degeneration—the result of advanced age, or of acquired weakness, either local or general. Dr. Jones's belief is, that the essential cause of this morbid change is the supply of unhealthy blood-plasma to the gland, by which a general deterioration takes place of all the most important textures. But can we state precisely what is the nature of the degenerative process, as illustrated by the constitution of the kidney, or that of the blood? It has been supposed that the change is essentially one of fatty degeneration, precisely analogous to fatty degeneration of the liver. Dr. Jones does not coincide with this view. He inclines to that of a German observer, Dr. Eichholz.]

He describes, if I remember aright, a condition of the general system, which might be termed the *fibrinous*, in opposition to the *tubercular diathesis*. In the former, there is a tendency to the formation of false membranes or serous surfaces, to the thickening and condensation of fibrous membranes, to such changes as produce thickened, stiff, puckered valves, white pericardial patches, cirrhosis of the liver, thickening of the capsule of the spleen, perhaps fibrous tumours in the uterus, and contracted and atrophied kidneys. The opposition between this diathesis and the tubercular, is well illustrated by cases of which I have seen a few, where old tubercular masses, or even single cavities, existed in the apices of the lungs, with surrounding induration and puckering of tissue, while some or other of the above-mentioned indications of the "fibrinous diathesis," were also present. The supervention of the tendency to fibroid formations arrested that to the deposit of tubercle. The above account is given from what I remember of Dr. Eichholz's statement, as I have not been able to find his paper again: I am pretty sure, however, that I have represented his views correctly, and my own observation has further inclined me to confide in their truth.

[With regard to some of the symptoms of Bright's disease, it is to be remarked, that the essential urinary constituents are retained in the blood, and often give rise to the symptoms of toxæmia. The specific gravity of the serum of the blood diminishes rapidly, falling from 1030, its natural specific gravity, to 1019 or 20. The most important change is that which strikes the eye of the practitioner in the well known sallow, whity complexion of the patient suffering from this disease: the proportion of red globules is remarkably diminished, sometimes to one-third of what it is found to be in health. This may be accounted for by the liquor sanguinis being in an unhealthy condition, blood corpuscles are not developed to their proper amount. This is clearly a degeneration of one of the most important constituents of the blood. The dropsy, which so commonly attends Bright's disease,

has been said to be the result of a too dilute state of the blood, or from diminished pressure on the walls of the vessels, or from ulcerations in the capillary vessels. Dr. Jones believes the first and last of these to be essential. The engorgement of the kidneys producing acute anasarca must be relieved—1st, by general bloodletting, if the patient's strength will bear it; 2nd, by cupping on the loins; 3rd, by tartar emetic, which, depressing the action of the heart, unloads the congested renal capillaries, and promotes the action of the skin. It is manifest that saline diuretics should not be used in this state of the kidney, but in the later stages of the complaint in moderate doses, they may be beneficial. What are the best means for restoring as much as possible the function to the degenerated organ? On this subject Dr. Jones remarks:]

Our guiding ideas are, I think, two; one recognising the degenerative nature of the disease, its essentially low character, and therefore seeking to oppose it by all possible corroborative means, and such as increase the general vigour of the system. Cod-liver oil, iron, quinine, regulated and active exercise, a system of training, such as an athletic employs, mental occupation; these judiciously employed in the earliest stages would, I believe, have much efficacy in counteracting the degenerative tendency. There is no doubt that the phthisical tendency, nay, the deposition of tubercle, in the lungs, may be checked, and life greatly prolonged, by such measures; why should we not employ them, also, with effect in this kindred degeneration? One can hardly help feeling that our duty, as practitioners, is not confined to the treatment of disease as it arises and excites alarm, but where the known diathesis, the exterior tokens of the inward infirmity, inspire us with too good ground for future apprehensions, we should give timely warning, and show how far easier it may be to avert the coming of the evil while yet distant, than to arrest it when its course has once begun. The second idea which would guide my treatment is the following, and is founded very much on the peculiar condition of the epithelium of the renal tubule in the earlier stages of degeneration. We know that the alkalies and several of their salts exert a remarkable influence upon albumen and allied matters, dissolving them or tending to render them more fluid. Now it seems to me quite conceivable that alkalies, either pure, or preferably, perhaps, as vegetable salts, which become decomposed ere arriving at the kidneys, may so act on the bulky, coarse, heaped-up epithelium, as to cause it to assume a more fluid state, creating less obstruction, and more ready to undergo its appropriate changes. Such an effect, or at least a similar one, was certainly produced by Mr. Brandish in his treatment of scrofulous glands by liquor potassæ; and I cannot but think we may derive a useful hint from the practice he so successfully pursued. Of course during the trial of such a plan, care must be taken to uphold the strength adequately, and not to push the remedy too fast. If this be not carefully observed, if the action of the remedy be not most carefully proportioned to the morbid process and to the general strength, it will cease to be a remedy; it will only act injuriously. I regret that I have not experience, to lay before the Society, of the result of such a plan of proceeding as I have traced. I have only employed it partially in one case; in this, indeed, all seemed to go on well for some time; the man, a gardener, who had Bright's disease in a marked form, kept to his employment, lost his dropsy, and seemed on the whole decidedly improved; but after some weeks the lungs became gorged and vomiting set in, which I was unable to restrain. Probably the renal degeneration was too far advanced, and the attendant circumstances were unfavourable; but still the results were sufficiently encouraging to induce me certainly to try the plan again when a suitable opportunity may occur. Cupping on the loins even Frerichs does not counsel in Bright's disease, a measure which, if the disease depended essentially upon exudation into the kidney, would seem certainly advisable, at least in small quantities, or frequently repeated dry cupping. It should only be employed when there appears to be an attack of engorgement supervening on the degeneration, and then as sparingly as possible.

I am inclined to think, from the condition of the urine in two cases which I have recently seen, that albuminuria may result simply from an altered and attenuated state of the membranous wall of the Malpighian capillaries. It is quite conceivable that this change in the vessels may have taken place without any coincident change *in the epithelium;* and indeed that this is possible, is proved by cases of so-called

chylous urine, in which after death the kidney has been found perfectly healthy. The case recorded by Dr. Bence Jones supports the same view, as no traces of epithelium of the tubes were found in the urine: liquor sanguinis and blood globules were effused from the Malpighian tufts, but the epithelium was probably unaltered, as the normal solid constituents of the urine appear to have been present. The influence of exercise in producing the effusion of liquor sanguinis in this case, and of gallic acid in restraining and arresting it, also indicate that the disease is essentially of the nature of a flux taking place from the attenuated Malpighian capillaries, much in the same way as it might from the capillaries of a mucous surface. The point that arrested my attention in the cases I have mentioned, was the perfect transparency of the urine, and its freedom from any cloudiness or sediment; this, I think, is not usual in Bright's disease.—*London Journal of Med., May* 1852, *p.* 401.

SURGERY.

FRACTURES, DISEASES OF JOINTS, AND DISLOCATIONS.

75.—ON THE TREATMENT OF FRACTURES OF THE FEMUR.

By Dr. Philip Bevan, Surgeon to Mercer's Hospital, Dublin, &c.

[Although fracture of the femur may be treated successfully by the modes at present in use, yet some slight deformity frequently results. There are two positions adopted, viz. the flexed and the straight. Dr. Bevan does not dwell upon the former, as he considers Dr. Houston's treatise fully established the superiority of the latter. The different forms of apparatus for maintaining the straight position, modifications of Dessault's splint, Dr. Bevan considers are all faulty. In the first place, in consequence of counter-extension being made with a bandage with different degrees of obliquity around the groin to the outside of the pelvis, a considerable degree of power is lost. 2ndly. As the counter-extension is made of a yielding texture, it requires to be replaced or re-tightened several times, thus disturbing the callus. 3rdly. The perineal bandage tends, in fracture of the neck of the bone, to separate the portion attached to the head from that connected with the shaft, and thus to keep at a distance the two broken fragments.]

The mode of making extension in Dessault's and in Liston's splints is liable to objections. First, the force is applied obliquely, and therefore there is a loss of power; and secondly, it has a great tendency to increase or keep up the rotation outwards of the limb, which this fracture usually produces. No doubt Boyer's mode of extension is a great improvement in this respect, but his screw is rather expensive and complicated. All the previous inventions require that the patient should lie on a perfectly resisting bed. This, in private, is of little importance, as we can use a hair mattress; but, in hospital, to lie on the hard straw mattress for six or eight weeks is by no means a trifling evil, and, in old persons, is either absolutely impossible, or very liable to cause excoriation or sloughing of the sacrum.

I propose a single, broad, strong splint under the entire limb, concave, and padded at the upper part, to pass below the tuber ischii. One perpendicular, strong bar of iron, well padded, rises from the inner border, and is intended to rest against the ramus of the pubes, whilst another similar, but longer bar, is placed also perpendicularly on a level with the spine of the ilium, on the outside of the splint. To the lower part of the splint a moveable perpendicular iron plate is attached by means of a thumb-screw. This plate is perforated in the centre, and through it an endless screw passes, to which a foot-board and shoe, well padded, are attached. The perpendicular plate is merely united to the splint by a screw, which can be fixed at varying lengths, so as to adapt it to the various size of the thigh at different ages; and also to allow, if necessary, all traction on the limb to be relaxed instantaneously, should circumstances require it. The counter-extension is here made partly by the perpendicular iron pressing against the pubes, and partly by the edge of the splint pressing against the tuber ischii. I at first feared that the pubes would be galled; but as the principal pressure is against the ischium, the friction is so slight on this part as not to give the slightest inconvenience. The perpendicular iron at the spine of the ilium is of use, in supporting the pillows, and fixing one which should always be placed under the trochanter major, to keep it in contact with the neck of the bone, and prevent it from falling backwards behind its natural site. I originally intended that this should press against the spine of the ilium, but the varying breadth of the pelvis rendered this difficult; and I found that pressure against the pubes and tuber ischii was quite sufficient without it.

The extension is made by the screw below, which is attached to an iron foot-board, and this may either be connected to the foot by a boot or a figure of 8 bandage, or, what answers better, by a circular bandage applied around the ankle, above the malleoli, with a strong band to connect it to the foot-board.

It might be supposed that this extension would gall the ankle, but the force required to keep up the extension, when applied thus in a right line, is so slight, that *little or no inconvenience* results.

A, represents a perpendicular perineal plate, well padded, and covered with chamois leather.
B, a similar one, placed opposite the spine of the ilium.
C, a moveable perpendicular plate, attached to the splint by a thumb-screw D, and capable of being moved, according to the length of the limb.
E, an endless screw, which is attached above to the foot-board of the boot, passes through an opening in the perpendicular plate, and can, by means of a nut placed below the plate, draw the foot-board down.

I need scarcely say how simple is the mode of adjusting this apparatus; in fact, having applied a bandage the entire length of the limb, the patient has merely to lie down on the splint, previously covered with a long pillow, with the pubes resting against the perineal plate, when, the extending bandage being applied, the ankle bound to the foot-board, and the screw tightened, the entire is complete. In a few cases of children, or unruly persons, a bandage to prevent the patient rising altogether off the splint might be required, but will rarely be necessary. A pad or pillow should be placed under the trochanter major to support it, keep it in contact with the neck of the bone, and assist in preventing rotation outwards.

The advantages which this apparatus presents are, I believe, numerous. 1st, It is remarkably simple. 2nd, The extension, or counter-extension, is made perpendicularly, and not obliquely, and therefore, a much less amount of force is required than by any other apparatus, no power being lost. 3rd, No pressure is made on the front of the thigh. 4th, The extension can be made very gradually. For the first few days, the limb may be left rather shorter than natural, and then it may be gradually lengthened by merely tightening the screw, without (as is necessary in Dessault's or Liston's splints) deranging or removing any other part of the apparatus. 5th, The entire limb is exposed to view, and should the fracture be compound, the tails may be removed without raising the limb, or diminishing the extension, and any shortening or deformity is immediately detected, which is not the case where the limb is surrounded by two or more splints, retained by bandages. 6th, As the extension is applied in a direct line, eversion of the foot is entirely prevented. 7th, The extension may be made above the malleoli, so as not to interfere with either the dorsum of the foot or the ankle. The traction on the ankle is a well-known evil, and has sometimes given rise to a relaxation of the joint, requiring several weeks for its removal. This mode of making extension is much better than that proposed by Mr. Bulley, in the form of a circular band around the thigh, above the patella; but in fact, after the first few days, the amount of traction is so slight, when used in the proper perpendicular direction as with this apparatus, that no injury can be done to any of the joints. And lastly, the counter-extension is principally made against the unyielding tuber ischii, a part well covered with a cushion of fat, and accustomed to pressure; the perineal plate, resting against the ramus of the pubes, sustains comparatively little pressure, and is of use chiefly in keeping the apparatus from slipping outward.

On first applying this splint, my patient suffered considerably from the traction on the ankle; but I afterwards found that this rose from using a degree of extension quite unnecessary. In fact, the muscles become partially paralysed by the gradual and slow traction, and after the first few days, the slightest force was sufficient to keep the limb to its normal length.

The splint will be invaluable in military practice, where a firm support under the limb is of so much importance; and in fractures of the condyles of the femur or tibia it will be equally useful; for whilst the extension is fully kept up, the knee-joint will be exposed to view, and this in a position favourable for the application of leeches or other treatment.

Cases of fracture of the neck of the femur within the capsule, when they occur in very old or debilitated persons, are best treated by Sir Astley Cooper's method, as, no doubt, any attempt to keep such patients for a length of time on their backs by any apparatus, however simple, must be followed by sloughing of the nates, in-

tative fever, and probably the death of the patient. But when this fracture occurs in younger and less debilitated subjects, I have some reason to hope that the occurrence of false joint will be obviated by the use of this apparatus, as two of the most usual causes of disunion, the too frequent motion, and the want of close contact of the fragments, will be hereby avoided.

That the too free motion will prevent union scarcely requires proof, and that the want of close contact of the fragments will equally prevent it is nearly as evident. Thus in fracture of the patella and of the olecranon, the want of contact alone would seem to prevent bony union, as there is no deficient vascularity of the parts, and bony deposits take place readily when the parts are in contact in longitudinal fracture of the same bones.—*Dublin Quarterly Journal of Med. Science, Feb.* 1852, *p.* 1.

76.—ON THE TREATMENT OF FRACTURES IN THE VICINITY OF THE ANKLE-JOINT.

WITH OBSERVATIONS ON THE PRACTICE OF TENOTOMY AS FACILITATING THE REDUCTION OF THE BROKEN BONES.

By Richard G. H. Butcher, Esq., Surgeon to Mercer's Hospital, Dublin, &c.

[The frequent and serious set of fractures and luxations of the bones forming the ankle-joint, presents us with very formidable conditions, demanding all the powers of surgery for their alleviation. In the 'Dublin Medical Press' for April, 1851, Mr. Butcher gave in detail an outline of a remarkable case of fracture of the internal and external malleolus, with dislocation of the tibia forwards. This case was treated on a particular plan, and by this the normal function of the joint was entirely preserved. In this case, immediately after the injury, the most striking features were the fore-shortening of the foot, the lengthened heel, and consequently increased arch of the tendo-Achillis. An inch above the lower margin of the internal malleolus there was a remarkable prominence, and so sharp was this upper fragment that it threatened to protrude through the strained and tightened integuments.]

On elevating the limb from the bed, the leg being bent on the tigh, the following solutions accounted for the deformity. The fibula was shattered two inches and a half from its lower edge, the pieces being very numerous and moveable; the internal malleolus was broken at its base, and as a sequence to the lateral support of the joint being lost, and probably in some measure to the force being continued, the tibia readily glided over the convex surface of the astragalus, while the foot, and the lower fragments, the internal and external malleoli, drooped backwards; not only was the tibia considerably advanced, but its inner margin was abnormally in front of the outer edge, or, more briefly, it was slightly rotated outwards. The articulating extremity of the tibia greatly overhung the navicular bone, yet, strictly speaking, its anterior edge did not rest upon it, for the foot was pointed downwards by the action of the powerful extensor muscles acting on the heel. Thus perfect dislocation of the tibia off the astragalus, as clearly indicated by manipulation, will account for the great fore-shortening of the foot, which fully amounted to an inch and a half. While an assistant flexed the leg upon the thigh, I was able without much difficulty, by traction on the foot, to lift it forward, together with the broken malleoli, into position, and then, by slightly inverting the foot, the lower fragment of the internal malleolus was brought up in apposition with its base, and the integuments, from being tense almost to bursting, were at once relaxed over the lower edge of the superior fragment; on removing the support for an instant, the exterior muscles, thrown into spasms, at once reproduced the deformity that stamped the accident. By a repetition of the above manœuvre co-aptation of the broken fragments was effected, and the limb placed in a fracture-box, the thigh slightly bent and the leg resting on the heel. The box was so constructed that it retained it in this position, for, while the thigh was supported on a gently inclined plane, the leg rested on a horizontal surface; the box was well padded, particularly beneath the heel; splints, with lateral foot-supports, were slid up between the cushions and the side of the box; each had a round hole bored in its *lower extremity, which* received the arms of the sole-piece; in addition to the lin-

ing pads of the splints for guarding against undue pressure, there were three used with the object of acting on the broken fragments and displaced parts; the first placed beneath the heel so as to press the foot and lower fragments well forwards; the second, somewhat in the form of a wedge, was placed along the inner side of the leg, the base of it resting about two inches above the fracture through the tibia; this tended to press the tibia outwards; while the third pad, also triangular, was placed between the outward side of the foot, and the corresponding splint, the base at the toes; this inverted the foot, and most effectually acted in bringing up the internal malleolus to the upper fragment. The splints and pads being adjusted in this way, the anterior surface of the limb was raised exactly to a height with the sides of the fracture-box, so that, when the first roller was applied from below the knee around the fracture-box and leg, as far as the centre of its lower third, and equable pressure was maintained all along the interior surface of the tibia, and by this means acted most efficiently in steadying the upper fragments, at the same time pressing them backwards; several layers of soft wadding were interposed to prevent any fretting of the integuments over the sharp spine of the tibia. The foot, as before mentioned, was kept well pressed forward by the increased number of pads under the heel, and fixed, *slightly* inverted, by a few turns of a roller, embracing the interior part of the dorsum of the foot, side splints, and foot board. By this arrangement the limb lay in admirable position, all deformity entirely removed, with the parts corresponding to the site of fracture, and ankle-joint freely exposed for local applications.

On the limb being done up in this way, the patient expressed himself as being relieved from all pain; a cold lotion was ordered to be kept to the part, and a full opiate was prescribed. On the following morning great swelling and effusion were present, the collapse having passed away, and re-action set in. I was at once careful to ascertain that no undue pressure was exerted by the mechanical means employed, but nothing could be more satisfactory than their adjustment. His bowels were freed by medicine: relays of leeches were directed to be applied, six at once, and the part to be afterwards enveloped in a warm stupe, so as to solicit a constant drain of blood; four more in three hours; and warm stupes to be continued all night. Though the local abstraction of blood was here imperatively demanded, taking into consideration the intemperate habits of the man, I did not think it prudent to stop altogether his usual stimulants, particularly after the infliction of so severe an injury. On the fourth day the nervous system began to give evidence of participating acutely in the local affection; he had scarcely any sleep, and that obtained was not perfect or refreshing, as he was awoke at short intervals by a tendency to spasms in the voluntary muscles of the upper as well as the lower extremities; pulse very irritable and rapid; respirations unequal; tongue tremulous, and covered with a whitish pasty coating. The limb lay in admirable position, sufficient restraint being employed to prevent deformity and guard against displacement, so as at the same time not to obstruct the free circulation in the overloaded vessels. He was now put under the influence of opium, and kept so. On the sixth day from the receipt of the injury he was better in every respect; he slept all night; his pulse quiet; no spasmodic startings; tremulous motion of the tongue subsided; and the limb free from pain, and in good position. On the twenty-second day after the accident I applied Dupuytren's splint and wedge-shaped pad along the back of the leg. This acted exactly on the same principle as the treatment adopted from the very first, after the receipt of the accident. The base of the pad resting on the splint behind forced the foot forwards, whilst the little pad, placed in front of the lower end of the tibia, and retained by a bandage, propelled backwards the articulating surface of the tibia. With the splint thus applied, after Dupuytren's directions, the limb was placed in the fracture-box, with side-splints as before, by which the foot was slightly inverted. On the 18th of April the union was quite firm; I therefore removed Dupuytren's apparatus; there was not the least fore-shortening of the foot, or displacement of any kind. I then put on a roller, side-splints, and foot-boards, as offering sufficient restraint; and on the 20th of the month he quitted the hospital, with the motions of the joint nearly perfect, and not the least trace of deformity.

The foregoing case offers, I conceive, a good illustration of what may be effected

by early co-aptation, and position, in the treatment of one of the most troublesome forms of fracture which the surgeon can be called upon to manage.

Case II.—*Fracture of the Tibia an inch above the ankle-joint, with that of the Fibula half-an-inch higher up.*

Patrick Grace, aged twenty-two years, a powerfully made, muscular man, was admitted into Mercer's Hospital, under my care, October 8, 1851. He had been wrestling, and received two very severe kicks on the left leg from his antagonist, who wore strongly nailed shoes. On admission, the deformity of the limb was very great, and characteristically striking of the lesion that had taken place. The foot drooped backwards, the heel was lengthened, and drawn up by the extensor muscles, which were in rigid contraction; at the same time there was an angle salient in front, formed above the articulation. This combination, then, rendered the displacement remarkable. On making extension at the foot, the leg being flexed on the thigh and held so by an assistant, the powerful action of the gastrocnemius was overcome, and a facility afforded of examining the nature of the fracture. By the force employed the foot could be brought up to its normal situation, which had the effect of entirely removing the deformity in front; and then by the slightest motion crepitus was elicited very audibly, and the tibia found to be fractured transversely an inch above the ankle-joint, and the fibula half-an-inch higher up. The integument over the part was much discolored from contusion. I at once placed the limb in a fracture-box, such as already described, the thigh semi-flexed, and the leg resting on a horizontal plane, while the foot, together with the lower fragments, was kept well pressed forward by pads beneath the heel. Side-splints were then slid up within the box, to which the sole piece was connected, thus maintaining the foot at a right angle with the leg; two bandages were then applied, as in the former case, and the region above the joint, the site of fracture, left uncovered for local applications; a full, warm opiate was administered, and all tendency to spasm of the calf in a short time subsided.

11th. He has had no pain or starting since the limb was put up; ordered cold to be applied to the injured part, and a full opiate to be given twice in the day.

14th. Up to this date the limb did not require to be disturbed; nothing could be more accurate than the co-aptation; scarcely any swelling about the part. On this day I applied Dupuytren's splint and cushion to the back of the leg, which effected equally well the apposition of the broken fragments; done up in this way the limb was placed in the fracture-box, with side-splints and foot-board as before.

[On the 30th the above case was dismissed, with all the motions of the joint healthy. The third case was one of fracture of the tibia and fibula two inches above the ankle. The fourth case was a transverse fracture of the tibia an inch above the ankle joint, with a vertical splitting of the lower fragment into the joint, and comminuted fracture of the fibula extending higher up. In the fifth case there was fracture of the tibia and fibula, an inch and a half above the ankle joint. The sixth case was a compound comminuted fracture of the tibia, two inches above the ankle joint, and fracture of the fibula a little lower down. Mr. Butcher has placed these cases on record on account of the recent introduction of tenotomy, the division of the extensor tendons to facilitate reduction, as practised by Meynier, Bérard, Laugier, and other French and German surgeons. On this point Mr. Butcher expresses himself as follows:]

I am of opinion that in ninety-nine cases out of a hundred, there will be no necessity for division of the tendon to effect reduction, if the limb be treated as I have advised; nay, on the contrary, I think, in some instances, the division of the tendon would be very injurious, as removing the support posteriorly from the ends of the broken bones, and thus permitting displacement in that direction. The mode in which the fracture-box, which I have described, supports the leg in a horizontal line, with the thigh slightly flexed, padded, and cushioned, as illustrated by the foregoing cases, meets every requirement of the surgeon. Dupuytren's splint, in conjunction with these means, as used in some of my cases, is the most admirable adjunct; but, taken by itself, it will not answer as well for the management of the form of fracture under consideration; for if the limb be done up as directed by Dupuytren, and placed flexed upon its side, some lateral displacement will take *place; or if, with the* splint so applied, the leg be allowed to rest upon the heel, it

is unsteady and rolls about, and the entire limb is in the extended position, a posture very objectionable, as making tense the tendo-Achillis.

From a review of these cases and the observations upon them, the following facts are, I think, deducible:—

First, that by proper position of the limb, and early reduction, co-aptation of the broken fragments can be effected, and spasm averted.

Secondly, As the result of the broken bones being kept in accurate position, irritation is subdued, excess of callus prevented, and the motions of the joint left unimpaired; a fact of great practical importance here, for the experiments of M. Cruveilhier prove that various forms of irritation will make the periosteum and ligaments ossify, and it has been ascertained that in some cases of fracture near the joints the ligaments have sometimes been converted into bone, and M. Rayer has observed, from numerous interesting experiments, that a similar change may be exerted not only in the fibrous but also in the cartilaginous structures.

Thirdly, That tenotomy is not called for in the vast majority of cases, being perhaps only admissible when permanent spasm has located in the extensor muscles, owing to neglect of early reduction.—*Dublin Quarterly Journal of Med. Science, Feb.* 1852, *p.* 96.

77.—TWO CASES OF INJURY TO THE KNEE-JOINT.

(Under the care of B. Phillips, Esq., and B. Holt, Esq., at Westminster Hospital.)

[In the last volume of the 'Retrospect' were noticed several cases exemplifying the injuries to which the knee-joint was liable, with their consequences and treatment. As the subject is of great importance, the following cases are subjoined as further illustrations.]

Case 1. *Fracture of both thighs and injury to the knee-joint.*—James H——, aged forty years, a brewer's drayman, robust and well-constituted, was admitted Sept. 9, 1851, under the care of Mr. Phillips, the patient having just been run over by his dray, which was heavily laden. Both thighs were found fractured about the middle, and the left knee-joint was opened by a lacerated wound, about half an inch in diameter. The man was cold and collapsed, suffering much pain, and had lost much blood from the wound, whence synovial fluid exuded *freely*. The limbs were temporarily laid in junks, (longitudinal sand-bags,) and stimulants administered. The latter contributed much in reviving the patient, and in a few hours the removal of the left leg, where the knee-joint had been opened by a lacerated wound, was proposed to the patient. He could not bring himself to consent to the operation. This measure, so evidently called for by the nature of the injury, was, therefore, not carried out. The right leg was fixed by the long splint, and the left placed into Amesbury's apparatus, a poultice being at the same time applied to the wound, and the thigh additionally secured by a leather splint in front.

From the 1st to the 14th day no symptoms of an unfavourable nature occurred beyond the natural effects on the system of the shock which followed so severe an injury. The treatment consisted principally in keeping the limbs at rest, with very slight extension, and in this the house-surgeon perfectly succeeded. Up to the 13th day the patient was given stimulants, gradually increased to half a pint of gin, two pints of porter, and beef-tea. By these measures, to which were added opiates at night, and occasionally during the day, the pulse, at first fluttering, acquired firmness and regularity. The opiates were, after the 14th day, gradually diminished to fifteen minims of tincture of opium, combined with half a drachm of aromatic spirit of ammonia at night.

The patient now slept well, and complained of no pain; the wound in the knee assumed a clean granulating surface, and was free from synovial discharges. The joint was but very little swollen, excepting a slight effusion beneath the patella and its ligament. The tongue, which was at first coated, became clean under the influence of two or three purgative doses, and carefully regulated stimuli. About one month after admission, the patient had a slight attack of gout, which readily yielded to the usual remedies; the sleeping draught was discontinued, and the gin reduced to three ounces. No medicines were ordered except a cough mixture. The knee progressed without a bad symptom, and at this period was healing

rapidly. Both legs were now placed on long splints, which restraint gave the patient no pain on the left side. The wound was ordered to be dressed with stimulating ointment; the general health was at the same time remaining extremely good.

Ten weeks after the accident, the wound of the knee was nearly healed, and both fractures firmly consolidated. The two limbs being compared, the left was found a quarter of an inch shorter than the right, and the lower fragment on the left side could be felt somewhat prominent on the outer and back part of the thigh. The knee-joint looked still large, but was free from tenderness. At this time both legs were bandaged, and the patient was allowed to sit up; he dressed, and moved off the bed on crutches, without inconvenience. On the 16th of December, thirteen weeks after admission, the patient was walking with a stick, and had perfect use of his joint.

Case II. *Incised wound of the knee-joint.*—James B——, aged 26, a cooper by trade, was admitted, June 23rd, under the care of Mr. Holt. It appears that while he was working with a draw-knife, the block slipped, and the knife wounded the knee-joint, making a transverse incision below the patella, dividing its ligament, and opening the joint extensively. On the patient's admission, the wound was immediately closed with adhesive plaster, and sealed with collodion, the limb being fixed on a splint in a slightly flexed position. These dressings being opened a few days afterwards, it was found that little attempt at union had been made, but the edges were carefully kept together until all hope of closing the wound had ceased.

In spite of the strictest antiphlogistic regimen, the joint became violently inflamed; large collections of matter formed, which required free incisions, and at the end of one month, the patient's condition was further complicated by an enormous bed-sore, which necessitated his removal to the water-bed. Previous to this change, the man had become so low that stimulants and nutritious broths were constantly administered.

On September 1st, nine weeks after admission, the patient was greatly reduced, and evidently suffering from hectic, night-sweats, and violent pains in the limb. The joint was, in fact, so exquisitely painful, that the slightest touch on any part of it gave excruciating pain. Purulent matter was discharging copiously from three openings, one of which was the original wound, and the two others were the result of the incisions which had been made on the outer side of the articulation. The leg was permanently extended, and the patella slightly elevated and moveable. Over the sacrum was a large, deep bed-sore, about three inches in diameter, fully exposing the bone. The appetite was very capricious, the principal sustenance being wine and beef-tea.

For three weeks the patient's condition did not vary materially, except that the sore on the back gradually diminished; the bowels were, however, very often confined, and the movements necessary for an alvine evacuation caused much suffering. From this time, the patient having now been about twelve weeks under treatment, the extreme sensibility of the joint rapidly diminished, and the man's strength and appetite began to improve, allowing him to take a little solid food. At the end of another week, a straight splint was by great care introduced beneath the leg, and the bedding changed. For a few days the fixed position gave great relief, but he soon began to complain of pressure on the leg; the bandages were therefore cut, and the limb allowed gradually to slip off the splint. When the leg was again extended on the bed, the patient expressed himself as feeling very easy; and with careful attention to his diet, he improved so much, that about four months after admission, a gutta-percha splint was applied, and he was removed to an ordinary bed. Previous to this, a sore had formed on the ankle, which complication gave much uneasiness.

When a little more than four months had elapsed, the wounds of the knee were all closed, and the form of the joint was not much altered. The patella was moveable, and it might be inferred that the other portions of the joint might be made to move, though from their sensitiveness, their capability in this respect could not be ascertained. The sores on the sacrum and ankle were at this period nearly healed. The leg and thigh were lying on their outer side, this being evidently owing to eversion at the hip, a natural effort intended to take the weight of the foot from the

knee. The patient was now getting stout, his appetite improving, and the functions were all normally performed.

As it was found that the gutta percha splint caused pain, it was removed, and the limb placed on pillows; the thigh and leg being of course very much wasted. Five months after admission, the patient's health had remarkably improved, and he was able by his own efforts to move the limb from side to side without experiencing any pain. On December 16, 1851, six months after admission, the patient had still some slight pain in the joint when the latter was moved incautiously, though all active mischief had passed away. The joint is now partially anchylosed.

The report of the foregoing cases, though far from being prolix, is of such an extent, that we are precluded from indulging in some of the reflections which the cases naturally suggest. We would merely mention that we are deeply impressed with the value of such histories, as showing the possible course of events in severe lesions of joints when amputation *is not performed*. Such cases are almost as instructive as would be the description of all the symptoms of some internal disease up to death or recovery, where no treatment whatever had been used. Will such histories as the ones above narrated lower the value of prompt measures? We apprehend not; but they will show how cases of injury to a large joint *may* now and then terminate. Had amputation been performed in either of the two instances, the operation would have been quite justifiable, and doubly so in the case of the drayman—a class of individuals well known for sinking very rapidly under acute inflammation of any important portion of the frame. But it is now plain that even a drayman has a chance under the circumstances above detailed, though it should be borne in mind that many patients of good constitution have rapidly died under the effect of much slighter articular injuries.—*Lancet, Dec.* 20, 1851, *p.* 580.

78.—*On Fractures into the Joint.* By SAMUEL SOLLY, Esq., F.R.S.—[Of the comparative danger of fractures extending into the joint, Mr. Solly observes,]

Of fractures into the joint, those of the tibia are, as far as my experience goes, the most dangerous. I have seen several simple fractures of the os femoris into the knee-joint, and they generally do well, but not always. Compound fracture of these parts is very rare. Mr. Travers relates a very interesting case in Sir A. Cooper's work. It occurred in a boy, though his age is not mentioned. There was much displacement of the fractured bone, and a small wound communicating. The fracture extended nearly in the direction of the axis of the bone, in addition to a transverse fracture of the shaft of the bone above the joint; the external condyle was moveable, and thrown out of its place during the accident, as if it had been drawn by the leg, and twisted inwards. There was very little constitutional disturbance. The fractured portion of bone ultimately protruded, and became detached. The accident occurred on the 17th of Sept., and he was discharged from the hospital on the 6th of Dec., being able to bend the joint and walk tolerably well with a stick. In the following February he had the free use of his joint.—*Lancet, Jan.* 10, 1852, *p.* 33.

79.—ON FRACTURE OF THE RIBS.

By JOHN HILTON, Esq., F.R.S., Surgeon to Guy's Hospital.

[In a clinical lecture upon this subject, Mr. Hilton remarks, that a fracture of the first and second ribs is more hazardous than that of the lower; and, again, that a fracture of the lower is more perilous than one of the middle. In discussing the treatment, the first point Mr. Hilton establishes is, the necessity of perfect rest; and the second point in the mechanical treatment is, that of bandaging the chest. In this part of the treatment, he says:]

I much prefer strapping to bandages, for the former has the advantage of steadying the ribs without impeding the respiration beyond what may be of use to the patient. It is applied by fixing strips of adhesive plaster, circularly from the spine to a little beyond the sternum, on the injured side only; leaving the other free, as well as on the abdomen on both sides. You perceived, by the details of

the last case, that the ordinary bandaging gave the patient uneasiness, and that he felt greatly relieved by the strapping. The application of the circular straps may be with advantage preceded by arranging from four to six straps on the chest, so that they cross each other over the seat of the fractures. This gives additional security against motion.

It is astonishing how admirably this system of strapping answers the purpose, and how distressed patients sometimes become with the common bandaging. I recollect being sent for to a patient, by Mr. Turner, of Bermondsey-square. Some ribs had suffered fracture, the case had been treated in the usual manner, and the bandage applied. Difficulty of breathing and general uneasiness became, however, so extreme that much alarm was created; I advised, in consultation with Mr. Turner, that the bandage should be removed: this measure gave the patient immediate relief. I had him then strapped in the manner just described, which rendered him comfortable, and with simple treatment by antimony he recovered perfectly, the strapping being persisted in. The same thing occurred in a case which I attended with Dr. Oldham. A stout, fat lady fell upon the corner of a table, and broke two or three ribs in her right side; a flannel bandage was applied; it caused great and increasing distress in breathing; it was taken off after a few hours, and I strapped the side: direct and continued relief was the result.

But not only is experience greatly in favour of strapping, but direct experiments have likewise proved the soundness of the method. Some experiments were conducted, at my solicitation, by Mr. Ewart, well known to you for his industry and talent, and I will just read you a short abstract of them:—

The subject was a carpenter, his chest well-formed, and evincing no abnormal signs on inspection, percussion, and auscultation.

Measurement during a Forced Expiration.	Inches.	*Measurement during a Forced or Full Inspiration.*	Inches.
Around the chest, and on a level with the lower border of the seventh rib. . . .	31	Around the chest, on a level with the lower border of the seventh rib	34
Both sides equal.		Both sides were equal.	
Around the chest, immediately above the nipples . .	34½	Around the chest, immediately above the nipples . .	37¾
Right side only . . .	17½	Right side only . . .	19¼
Left side only . . .	17	Left side only . . .	18¾

Capacity of the chest when free, as shown by the spirometer, 250 cubic inches.

	Cubic Inches.		Cubic Inches.
When the right side was strapped with adhesive plaster .	230	When a roller-bandage was applied around the whole chest	200

Measurement with Strapping applied to the right side of the Chest during a Full Inspiration.

	Inches.		Inches.
Around the chest, on a level with the lower border of the seventh rib. . . .	33¾	Around the chest, immediately above the nipples . .	36½
Right side only (strapped) .	16¾	Right side only (strapped) .	18
Left side only . . .	17	Left side only . . .	18¼

The respiratory murmur on the strapped side was obviously diminished, as well as the mobility of the ribs.

Thus, you see, a patient with this peculiar support upon the injured side, or on one side, breathes twenty cubic inches less of air than when the side is free from strapping, showing distinctly the limiting effect of the strapping as regards the movements of the ribs. You will also notice that when the chest is bandaged tightly in the usual way (and the bandage must be tight to be of the intended *service), the* patient is robbed of fifty cubic inches of his breath. This is a serious,

distressing, and unnecessary deprivation, and can be avoided, as the experiments show, not only to his comfort, but without any disadvantage towards keeping the ribs in a state of rest. We have not only experience and experiments in favour of this kind of support, but, in reasoning on the subject, I think you will likewise find it the more satisfactory plan. Just consider, that by applying a tight roller in the usual way, you confine the chest by a nearly unyielding circle. What must happen at each inspiration? Why, the ribs on the sound side lift up or carry the bandage outward on that side, and in the same proportion that it is carried outwards on the uninjured side (it being itself unyielding), so must it be forced inward on the injured side, pressing and thrusting the broken ribs inward upon the pleura and lungs. Again, is it not probable that strapping the ribs to each other may have the effect of overweighting the first and second ribs with their muscles, and so prevent the very beginning of the series of successive costal movements from above to below, which occur in ordinary full breathing? for it is clear that each pair of intercostal muscles is constructed in reference to the size and weight only of the two ribs to be moved.

A still worse practice is to apply the above-named bandage or roller both to the chest and abdomen, for by confining the action of the parietes of the latter, you throw a serious obstacle in the way of, and may be, fatally deprive him of abdominal or diaphragmatic respiration, which in cases of fractured ribs is, as it were, vicarious of the thoracic; the descent of the diaphragm will be materially interfered with, and the patient may thus become much distressed, especially after drinking or taking food into the stomach.

Thus you see that, first, experience—second, experiments—and third, reasoning—are all in favour of the strapping system which I have used in the cases related, and which has yielded in most cases the most satisfactory results.—*Lancet, March* 13, 1852, *p.* 255.

80.—TREATMENT OF GUN-SHOT FRACTURES.

By G. J. Guthrie, F.R.S.

[On this subject, Mr. Guthrie lays down briefly the following precepts:]

1. An upper extremity should not be amputated for almost any accident or accidents which can reasonably happen to it from musket-shot.

2. If the head or articulating extremity of the bone entering into the composition of the shoulder-joint should be broken to pieces, that portion of the bone should be sawn off, but the arm must remain.

3. If the elbow-joint is shot through, it is to be cut out, and the forearm brought into the bent position. The sufferer will have a very good and useful arm, &c. I have, however, added that if the surgeon does not know how to do these operations he had better cut off the arm.—*Lancet, Jan.* 31, 1852, *p.* 114.

81.—TREATMENT OF DISEASED JOINTS BY INCISIONS.

By John Gay, Esq., Surgeon to the Royal Free Hospital.

[Mr. Gay observes that]

To the present time there is no department of surgery in which the powers of art have been comparatively so feeble as when applied to the relief of those diseases of the joints which, from their results, might be termed destructive. Hence, let the articular surfaces of the joint be bereft of their cartilages, a sinus or two be formed around it, and the health of the patient show symptoms of exhaustion, and the joint, and probably the whole limb, is doomed to amputation. He adverted to the causes of the removal of the cartilage from joints, and gave it as his opinion, that in addition to primary, synovial, and osseous disease, the cartilages were sometimes removed by absorption, in consequence of degeneration of their own tissue, without any traceable affections of the contiguous textures. In all cases of removal of cartilage, the tissue degenerates into a kind of fibrous texture, antecedent to the final process; and as portions of cartilage were sometimes

observed to be removed without any apparent disorder of either the synovial or osseous surfaces, and, moreover, as cartilage was known to be inadequate to its own repair, Mr. Gay thinks it most probable that the portions of cartilage so removed had first spontaneously degenerated, and then become absorbed. Mr. Gay went on to remark, that if a series of joints be examined in which the removal of the cartilages is taking place, the appearance will be as follows:—If it be presumed to follow disease of the synovial capsule, the cartilage will be found in some to maintain its connexion with the bone, while it is thinned by absorption at its free surface. In others, however, the bone is found inflamed at various points of its connexion with the cartilage; and at these points the cartilage is loose, and may be peeled off, so that portions of thin attached and unattached cartilages are found in the same joint. When entirely denuded, or almost so, the surface of the bones may exhibit simply a state of increased vascularity, which precedes the effusion of plastic lymph for the purposes of reparation by anchylosis, or may be observed to be in a condition of ulceration. This ulceration may exist as a simple abrasion, or be of considerable depth, but there is generally an uniformity in this respect over the whole surface. With this state of ulceration there is also a softening of the osseous structure, and frequently disintegration; the contents of the joint consisting of broken-up cartilage and osseous and other *débris* together, or osseous matter with ichorous or sanious discharge. When the disease originates in the bone, as in by far the greater number of cases in Mr. Gay's opinion it does, the separation of the cartilage is effected by another process, which he terms "shedding," and the cartilage is then reduced to the condition of a foreign body within the joint. Shreds of cartilage thus situated in a joint may be observed after months and even years of disease; and as on the other hand its separation from the articular extremity of the bones may be accomplished in an almost incredibly short period of time, it is fair to infer that the time thus passed must have been occupied in the process of its extrusion from the joint, and that this is accomplished, neither by ulceration nor absorption, but disintegration by, and solution in, the discharges of the joint. But the bone itself being diseased, adds its exfoliated or disintegrated particles to the cartilaginous débris, which, with its own discharges, constitute generally the contents of a joint in which the disease commenced in its bony elements. The result of these discharges is to set up inflammation in the sound textures contiguous to the joint, and general systemic irritation. Sinuses form around the joint; the disease extends itself; the ligaments become ulcerated; the spongy tissue of the bones infiltrated with pus, and broken down; osteophytes form around the heads of the bones; abscesses extend themselves into the surrounding soft parts, separating the different structures, and setting up unhealthy and destructive action among them; and, in short, a climax is arrived at in which the local mischief reacts upon the constitution, and life is only to be preserved at the sacrifice of the joint or the limb. Mr. Gay inferred from these remarks, of which only an imperfect abstract has been given,—

1. That there appears to be no reason why diseases affecting the constituents of a joint should be slower in their course of reparation than diseases of any other part or structure.

2. That the removal of cartilage from its osseous connexion in a joint is occasionally effected by absorption, but most frequently by a process of "shedding" or exfoliation.

3. That cartilages thus shed become, by their being pent up in a joint, sources of local and constitutional irritation, and thus promote disease in the osseous and other structures appertaining to a joint, supposing that such affections do not exist primarily; and in case they do, these cartilages, by the same influence, maintain and extend these diseases also.

4. That the natural outlets for these discharges, the sinuses, are inadequate for that purpose.

5. That therefore the exfoliated contents of a diseased joint have to be minutely broken up by, or dissolved in, the discharges of the joint, in order for their removal; processes which are necessarily of a very protracted order, and which account for the tardiness in general characteristic of joint-diseases.

6. That the exfoliated contents of a joint, after its cartilages have been removed, *and even after extensive* disease has been set up in the bones and other textures,

have only to be completely removed, and processes of reparation will, in the majority of instances, immediately commence.

Mr. Gay then alluded to the usual modes of treatment, and remarked, that the operation of re-section of a joint is not only a useless but an unphilosophical mode of treatment for diseased joints. In the first place, primary disease is generally limited to one of the articular extremities of the joint; it is, therefore, a useless mutilation to remove more than that disease, supposing the operation were for a moment admissible. But, moreover, dissections show that disease originating in bone, when arrived at that stage at which the operation of re-section is generally employed, has extended itself far beneath the surface, and frequently along the shaft for a third of its whole length, so that re-section cannot accomplish its purpose, which must be manifestly the removal of all disease. The plan Mr. Gay recommends, then, is free and deep incisions made along each side of the joint, so as to lay open its cavity freely, and to allow of no discharges being by any possibility retained within its cavity. They should be made of such a length, and so treated that they do not heal into the form of sinuses. They should be made, if possible, one on either side of the joint, and in the direction of the long axis of the limb. They should extend into the abscesses in the soft parts, so as to lay them open. If sinuses exist, the incisions should be carried through them, if this can be done without departing from a slight curve. If either of the bones be carious or necrosed, the incisions should be carried deep into such bones, so as to allow the dead particles of bone to escape. Ligaments which stand in the way of a free discharge from the joint should be cut through. Of course important vessels should be avoided. The wounds should be kept open by pledgets of lint, and free suppuration encouraged. The constitutional powers have in each case rallied immediately after the operation; and as the discharges from the joint have altered in character and become healthy, which they in general do in the course of two or three weeks, these become invigorated, and improve with the improving joint. Mr. Gay then narrated some cases in corroboration of his views:—Peter D——, aged thirty-eight, admitted into the Royal Free Hospital in 1842 for diseased elbow-joint of three years' standing, with ulceration of the cartilages and sinuses. The joint was opened on either side, and healed in eleven weeks. The next was a case of disease in the articulation between the first and second phalanges of the thumb, of eighteen months' standing. Cured in six weeks. The third case was that of a man with "long standing" disease of the tarsal articulation. One sinus led to the interior of the joint. Incisions were made on each side of the foot, and complete repair by anchylosis followed. The fourth case was that of a little boy with strumous constitution, with disease of the knee-joint consequent upon suppuration of the bursa behind that joint. The little fellow was reduced by fever to a very low ebb, so that bed-sores formed on parts of his body. The joint was opened, and anchylosis took place at the end of four months, the knee bent on the thigh. The fifth case was that of a German, with disease of the wrist-joint, which had resisted treatment. One sinus led into it. One incision was made at the back of the joint, and anchylosis followed, but was not observed to be perfect for six months. The sixth case was that of a young Irishwoman, with disease of the tarsal articulation, following upon traumatic erysipelas of the leg and foot. She was reduced to an exceedingly low condition, and from cough with bloody sputa, night-sweats (according to Dr. Heale), the physical symptoms of the chest, and extreme emaciation, she was supposed to be phthisical, and so diseased, that amputation, which was supposed to be the only remedy for the disease, as far as the joint was concerned, was forbidden by the authority of Dr. Heale. Mr. Gay made an incision on either side of the foot in this case, and the change both in the joint and constitution was remarkable. Her health rallied from that moment, and the joint assumed a more healthy aspect. In a fortnight the joint was fixed by the exudation of lymph between the bony surfaces, and in five weeks perfect anchylosis had taken place, and the wounds had healed. She soon afterwards left the hospital, and was a week or two since, to Mr. Gay's knowledge, in perfect health. The seventh case was that of Highley, reported at the time in "The Lancet." The eighth case was that of a little boy with disease of the articulation of the first and second phalanges of the thumb. In this case the cure was not accomplished. The incisions re-

solved themselves into sinuses, and after several months the necrosed phalanx came away.

Mr. HANCOCK had as great a dread as any one of opening joints under certain circumstances—as when, for instance, they were healthy; but it was quite a different matter when the joint was diseased; there was then no danger from cutting into it. He did not, however, see the novelty of the proceeding advocated by Mr. Gay. He (Mr. Hancock) had been long in the habit of opening joints when they were in the condition described in the paper. He had been struck with the kind and character of the joints which had been operated upon by Mr. Gay: they included the elbow, the ankle, the carpal, and the metatarsal joints; but he had brought forward no instance in which the larger joints, as those of the knee and hip, had been submitted to the proceeding. He (Dr. Hancock) had lately a case in which he had made incisions in the foot from one end to the other, on both its dorsal and palmar aspects, to remove the matter which had collected there. He had also frequently laid open the elbow-joint. Indeed, he thought we might do almost anything with the elbow or shoulder joints. He had made large incisions into the latter. When necessary to make an incision in these cases, he considered a free incision as much better than a small one. With respect to the length of time which diseased bones took to heal, he thought this depended mainly on the part where the disease was situated: when it was in the shaft, surrounded by muscles, it might quickly heal; but when surrounded by synovial membrane, as was the case with the knee-joint, it was a long time in getting well. He agreed with Mr. Gay as to the value of the practice which he had advocated; still it was very important that the incisions should be made in the most depending position, so that the matter might drain away. This was important, for a very small quantity of matter left in the joint might destroy the patient, by producing constitutional irritation from decomposition of the confined matter. He had never had a success equal to Mr. Gay, *quoad* the rapidity of the cure, the recoveries in his (Mr. Hancock's) case being always more protracted.—*Lancet*, *Nov.* 22, 1851, *p.* 494.

82.—ON THE EXCISION OF DISEASED JOINTS.

By MR. HANCOCK.

[The following remarks were made in a paper which Mr. Hancock read before the Medical Society of London.]

Mr. Hancock alleges that in these cases the degree of suppuration does not influence the patient so much as the character of the discharge. A patient with diseased joint, discharging but little, but that little of a morbid and unhealthy character, will suffer from extreme constitutional disturbance: he will become emaciated, lose strength, and be reduced almost to the verge of the grave. Operate upon this joint, remove the diseased parts, and let the subsequent discharge be ever so profuse, so long as it is not actually of a morbid character, and the constitutional symptoms will subside, the patient will gradually acquire strength and flesh, whilst the improvement in the character of the discharge marks the gradual progress towards recovery; that this is accounted for when we consider, that in the one instance there is a morbid action going on in diseased structures, producing unhealthy discharge, whilst in the other the process is one of reparation, proceeding in healthy parts, and giving rise to healthy though profuse discharge; that sinuses do not form so readily after these operations as has been imagined; and that when they do occur, they are more attributable to bad management and dressing, than to the actual necessities and nature of the case; that as a general rule they may always be prevented, if reasonable attention be paid to the preservation of a free and depending opening for the exit of matter throughout the progress of the case, such opening to be preserved in the dressing and splint, if any be used, as well as in the wound, which at this part should be left entirely uncovered; whilst he denies that the shock is greater than that attending an ordinary case of amputation. That with regard to Mr. Gay's objections, "that the operation for excision of joints is useless and unphilosophical, that primary disease is generally limited to one of the articular extremities of the joint, and that therefore it is a useless mutilation to *remove more than* that disease, and that, moreover, dissections show, that disease

originating in bone, when arrived at that stage at which the operation of re-section is generally employed, has extended itself far beneath the surface, and frequently along the shaft, for a third of its whole length, so that re-section cannot accomplish its purpose, which must be manifestly the removal of all disease,"—Mr. Hancock observes, that the operation of re-section is not an useless operation, is proved by the cases just related, as well as by those published from time to time by Messrs. Syme, Fergusson, and others; and that it is not unphilosophical, is also amply proved by the fact of its being an operation of preservation, and a substitute for amputation; that a surgeon is not compelled to remove sound parts; neither is an operation less one of re-section of a joint, because one of the articulating surfaces of such joint may be found healthy, and it may be deemed advisable to preserve it. As regards the condition of parts disclosed by dissection, Mr. Hancock would inquire whether such dissections were made upon parts removed by this operation? or were they made upon cases selected by the author, and decided upon by him as those upon which the operation would be performed? He was not aware that surgeons were in the habit of operating, with the almost certainty of leaving a large portion of disease behind them: and he would submit, that this very objection to the operation is a convincing proof of its efficacy; for if it has proved so successful when performed in so bungling a manner, and under such disadvantageous circumstances, how much more certain and beneficial will be its results when properly done, and performed at the right time; but he denied that the parts were usually in the condition asserted by Mr. Gay, when the operation for re-section was performed. Cases did occasionally occur, in which the disease has progressed to such an extent, or even beyond; but these were exceptions, and he would refer to three of the preparations upon the table, which clearly demonstrate, that although the disease shall be sufficient to destroy the patient, the state of parts will admit of the entire removal of the morbid structures by the operation of re-section, without any undue difficulty or mutilation of parts. If the question at issue rested simply between incising and excising joints, he would not for one instant deny the superiority of the former over the latter proceeding; but in those cases justifying excision of the joints, the selection is not between that operation and making free incision into the joint, but between amputation of the limb and excision of the joint, or leaving the patient to die. Mr. Gay limits his method to those cases where the articulations are completely destroyed; and whilst Mr. Hancock admits that Mr. Gay has done good service in directing attention to this plan, he would observe, that when reduced to this condition, the parts can scarcely, be regarded as joints, since they no longer possess the constituent structures of joints; whilst in a large proportion of cases requiring amputation or excision, the destruction of parts has not attained this state; that the necessity for such operations does not depend so much upon the extent of destruction or disorganization, as upon the degree of constitutional disturbance attending the progress of such disorganization; for in many instances the severity of the constitutional symptoms has necessitated amputation where the degree of disorganization has been comparatively slight, proving that the urgency of cases depends upon the powers of endurance in the patient, rather than upon the actual extent of disease or destruction of parts; so that whilst he readily admitted, that when completely disorganised—when the cartilages were entirely denuded and destroyed—incisions into the joint might prove of avail, he would urge, that in an equal, if not in a more numerous class of cases, the irritation produced by the progressing morbid changes is so excessive, that we cannot depend upon such simple means, but are forced to have recourse to the more decided plan of either amputation or excision.

Mr. Gay remarked that his paper had related to the incision and not the excision of joints, which latter was touched upon rather incidentally than in detail; and if his views had been more fully given, they would probably have been somewhat modified. He still, however, supported the views which he had advanced in the paper, and regarded the operation of excision as unphilosophical, and one which rather improved the art than the science of surgery. He denied that the success of an operation was *per se* to be taken as evidence of its utility or its soundness, and said, if such were the case, amputation for aneurism might be said to be good, because it effectually removed the disease. Excision was a sort of amputation, therefore a mutilation, and he objected to it as a substitute for an operation founded on the established

principles of pathology. He contended, by reference to cases, that excision of the joint had been practised without reference to the origin of the disease or the condition of the parts, and hence in some cases the operation had been unnecessarily performed. He objected also to excision, on the ground that no sufficient distinction was drawn between the primary and secondary diseases of joints, and he showed the difficulty there would be in knowing when you had removed the diseased parts to a sufficient extent to determine that a cure would follow. He did not think that the result of excision had been so favourable as some had said, and believed that bad cases had been suppressed. He then referred to the statistics of Blackburn, Norman, Walton, Syme, and others, to show that, with the exception of cases of the elbow-joint, excision had not been even a moderately successful operation. He then proceeded to express his opinion that the disease of the joints should be treated on the same principles as disease of the shaft of a bone—necrosis, for instance—and only such portion removed as was necessary to the recovery of the parts. He thought excision had been performed indiscriminately, and without sufficient reference to the structure in which the disease originated.—*Lancet, Jan.* 17, 1852, *p.* 81.

83.—ON EXCISION OF JOINTS.

By G. M. Jones, Esq., Surgeon to the Jersey Hospital.

[Sarah Hansford, aged 25, was admitted into the Jersey Hospital on the first of January, 1851, on account of a disease in the knee. When nine years old, she had an inflammation in the knee, arising, it is supposed, from exposure to cold. Since then it has scarcely ever been free from pain, and has always been considerably larger than the right one. The catamenia came on at 16, and has been regular since. In April, 1848, she was admitted into the Reading Hospital, and the report of her case from the books of that Charity is as follows: "Disease of left knee, involving synovial membrane, probably cartilage, and also ligamentous and cellular tissue; knee much enlarged, the slightest movement gives pain."]

Present Appearance.—The affected joint is very much enlarged, more particularly on the inner part; it is exquisitely tender on pressure. The swelling throughout is elastic and glossy, and the superficial veins very much enlarged. The knee presents, in an aggravated form, the appearance described by Mr. May. For the last few months she has suffered more in it than she had done before; she sleeps but little, in consequence of frequent lancinating pain; and this sleep is unrefreshing. She has scarcely any appetite. Pulse ranges from 100 to 105; has lately perspired freely towards morning. States that she has become much thinner.

As there could not exist a doubt respecting the nature of the case, or that the disease was progressing rapidly, it was decided, in consultation, that, in consequence of the absence of any very formidable symptom, this was possibly a case in which excision of the joint might prove successful; and, as the patient was willing to submit to any operation or treatment save the removal of the limb, she cheerfully acceded to a proposal which her repugnance to losing her leg had in a great measure suggested.

On the 19th, one week after the cessation of the catamenia, the operation was performed (the patient being under the influence of chloroform) in presence of several of my medical friends, some of whom kindly assisted me on the occasion. The patient was so placed on the table as to allow the leg and the greater portion of the thigh to hang over it. As a very minute examination, together with the appearance of the knee, indicated that extreme disease existed, I made my lateral incisions midway below and above each side of the joint, about five inches in length, cutting at once down to the bone. These were united by a transverse one, carried across immediately over the centre of the patella. The flaps were then dissected backwards and forwards, and the patella, which was soft and spongy, removed; the surrounding soft parts of the femur were then cautiously detached as high up as disease appeared to exist, and the crucial and lateral ligaments were divided; this allowed the assistant who had charge of the leg at once to bend it backwards, while the other raised the thigh from below upwards. By *this means considerable* facility was afforded for clearing the posterior part of the

femur of its different attachments. The joint thus exposed discovered the cartilaginous surfaces of both femur and tibia destroyed by ulceration, and a considerable portion of the osseous surfaces in a state of caries. There was pus external to the joint, as well as in the joint itself. The removal of the diseased part of the femur was accomplished with the common amputating saw; a large portion of the head of the tibia was also taken off with the same instrument. The fibula was found sound; neither spatula nor retractor was used. The hemorrhage was very trifling; no vessel required a ligature. The length of bone removed was four inches. The edges of the wound were now brought together with sutures and a few adhesive straps, the bones placed in juxta-position, cold-water dressings applied, and the limb secured in a modern apparatus (in some respects similar to Sir Astley Cooper's fracture-box). The whole of the operation occupied twenty minutes.

The patient returned to consciousness very soon after being placed in bed. The pulse was extremely feeble and correspondingly quick, extremities cold, and features somewhat attenuated. She evinced much surprise on finding that the operation was over. Warm flannel was applied to the hands and feet, and brandy and water given. The pain being excessive two hours after the operation, fifty drops of tinct. opii, in camphor julep, were taken. She has experienced much nausea, and has vomited twice, evidently the effects of chloroform, as similar symptoms occurred three days before, when its effects were tested on her.

9 p.m.—Still suffers much pain, and is inclined to be restless, but not to the same degree as before the anodyne. Has taken nearly a pint of strong beef-tea, with pepper in it.

The anodyne (same quantity) to be repeated.

Midnight.—Has slept by snatches since the last visit; pulse 115, and weak; much less nausea.

Beef tea to be continued, and port wine and water to be taken occasionally.

20th, Mane.—Has slept about two hours, and altogether feels more comfortable. No heat whatever of the limb. Pulse 110, very feeble; tongue moist, but rather coated. No return of vomiting or nausea.

A glass of port wine every three hours, each glass to be preceded by one ounce of quinine mixture equal to two grains per dose. Beef-tea *ad libitum*.

Vespere.—Pulse 106; feels comfortable; but little oozing from the wounds; some tenderness, but only on pressure; tongue moist. The affected leg and foot the same temperature as the other. The kidneys act freely.

Feb. 28.—To have continued a daily account of this case from the last report to the present time would have been a tedious recapitulation of almost the same appearances and symptoms, together with the same treatment as have been already noticed. For the first eleven days after the operation, there was not a single unfavourable symptom, not one to cause the least uneasiness for the patient's life; and since that time she has progressed satisfactorily—perfectly so, as far as her health is concerned, for it is now considerably better than it had been for many months, and favourably also as regards the appearance of the knee; the tumefaction naturally resulting from an operation of this nature has considerably subsided; there exists scarcely any œdema of the leg; and, although the discharge is considerable, and evidently from sinuses both above and below the parts once forming the knee-joint, still it is of a healthy character. Pressure on the parts can now be borne with much less pain than formerly. The appetite has also improved. The bowels are constipated (as they have generally been); care, however, has been taken that they should be relieved every second or third day. The renal secretion continues natural.

The same dressings to be continued, and also the same nutritious diet. Quinine and port wine as usual.

March 20.—Felt feverish during the night, and experienced several rigors. Complains of throbbing pains rather better than midway down the thigh. Two openings in the fore part of the knee have been closed for the last few days, and the pain complained of evidently arises from pent-up discharge; in other respects, everything is going on favourably; pulse is rather accelerated—90 (till now it has ranged from 76 to 84); some thirst; tongue clean, countenance cheerful; bowels as usual.

Porter and quinine to be omitted; saline aperient to be taken immediately, and the hot-water dressing with oil-silk, etc., to be applied over the fore-part of the thigh, and transverse incision of the knee.

21st.—Feels comfortable this morning, and slept tolerably well the latter part of the night. The aperient acted twice. Pulse 80. A considerable quantity of healthy pus has made its exit through the former openings. Tenderness of the thigh has almost altogether subsided. Has for the last week complained occasionally of pain in the back, arising most probably from the catamenia not having appeared.

Hot-water dressing to be continued. Wine and porter as before. The quinine to be omitted, and ℥j of mist. ferri. com. to be taken twice a day.

31st.—Health continues to improve; appetite very good. There is much less discharge from the knee; nights are very good; catamenia has not yet appeared.

Diet as usual; medicine also.

April 10th.—The catamenia came on yesterday. The appearance of the knee continues favourable; feels weak; pulse is, however, good, and everything connected with the digestive organs healthy.

Omit medicine. Food of the most nutritious kind to be continued.

30th.—The leg was taken out of the box to-day, and gutta-percha splints, moulded to the shape of the knee, applied. The wounds have all healed, with the exception of one place about the size of a sixpenny piece, situated at the upper part of the inner lateral incision; from this there still continues a discharge, but by no means considerable. Complete bony union has not taken place, as there exists some degree of flexion and extension; she can, however, without any other assistance than that given by the splints, raise the whole limb, and keep it raised for some time. Her health has wonderfully improved.

May 15th.—Has been down into the open air every day since the last report, and finds herself stronger than she has been for two or three years past. The small wound has not healed, but looks healthy; with the assistance of one crutch and a high-heeled shoe she is able to walk very tolerably.

24th.—Was able to walk across the room without any assistance; and, notwithstanding my persuasions to the contrary, at the instigation of her husband she left the hospital. On July 17 she was re-admitted. During her absence she had had many hardships to contend with: the ill-usage of a drinking husband, the sole care of a family, and scanty provisions; which, together with neglect of dressing the knee, and the being obliged to move about at every hour of the day, necessarily produced considerable mischief; so that, instead of the small healthy wound which existed when she left the hospital, and which, had she remained there, would, I feel convinced, have disappeared within a month, I had now the mortification of finding several small sores communicating with sinuses, and a considerable discharge flowing from them. Perfect rest was again enjoined, and this, together with nourishing food and malt liquor, gives me every hope that her imprudent step will have no worse result than that of retarding the cure. The knee, at present, is almost entirely healed, and again (with the assistance of a very ingenious frame made by a gunsmith of this place) she is able to walk with a stick; and I confidently expect that ere long she will do so without this adventitious help. She has not a single ailment, and is much stouter than she has ever been before.—*Med. Times and Gazette, March* 20, 1852, *p.* 287.

84.—*On Dislocation of the Thumb.* M. Roux's method of Reduction.—M. Demarquay has published in the *Bulletin de Thérapeutique*, certain experiments of his which were undertaken to ascertain the actual state of parts in luxation of the thumb. The case which directed M. Demarquay's attention to the subject, is the following:—A lady, in leaving her carriage, fell upon the pavement with outstretched arms, and a dislocation of the thumb ensued. M. Demarquay thus describes the state of the parts:—The thumb was forced backwards, and formed an obtuse angle with the corresponding metacarpal bone, whose phalangeal articulation projected under the muscles of the thumb. The ungual phalanx was flexed, *and all movements* of further flexion or extension were impossible. M. Demar-

quay used all known means of reduction without success, and M. Roux was called in previous to muscular section. The latter surgeon used the same tractions as had before been done; but before flexing the thumb he rotated it inwards, whilst he made forcible flexion, and reduction was thus obtained.

M. Demarquay was struck with this result; he made several experiments on the subject, and found that in a complete luxation of the thumb backwards, the following changes take place:—The metacarpal extremity of the first phalanx comes to rest on the posterior portion of the articular surface of the first metacarpal bone; and the phalangeal extremity of the latter projects under the skin, after having passed through the two portions of the flexor brevis pollicis, the external portion being frequently torn. This phalangeal extremity of the first metacarpal bone is thus caught in a loop formed externally by the outer part of the flexor brevis and the abductor, and internally by the inner portion of the flexor brevis, the abductor pollicis, and the strong tendon of the flexor longus pollicis. That these are really the phenomena which take place has been ascertained by the experiments of Messrs. Pailloux, Vidal, Malgaigne, Lisfranc, and Laurie. Inspections of individuals who have died with this luxation have shown besides that the anterior ligament is always ruptured close to the metacarpal bone, and that it is carried along with the posterior portion of the luxated first phalanx. As in the above-mentioned experiments both lateral ligaments, or at least the external, were always ruptured, the theory of Dupuytren and Rey falls to the ground. If, then, the difficulty of reduction cannot be ascribed to the persistence of the lateral ligaments, nor to the force of the anterior ligament, the main obstacle must lie in the muscular loop, which is slung round the head of the metacarpal bone.

In summing up, Mr. Demarquay gives the following rules: 1. To use a sufficient amount of traction (with Charrière's forceps) on the luxated bone, in the direction of the axis of the thumb. 2. To push back with the operator's thumb, or left index-finger, the head of the metacarpal bone, and keep it quite steady, so that when flexion is made, the head may not be thrust further into the palm of the hand. 3. When extension is carried to a sufficient extent, rotation either towards the outer or inner side should be made, so that the head of the metacarpal bone may be freed from the muscular loop which is keeping it strangulated. This being done, the traction should be continued until the posterior part of the first phalanx has reached the level of the head of the metacarpal bone. Flexion is then to be made, whilst at the same time the left thumb of the operator forces backwards the displaced part of the first metacarpal bone.—*Lancet, March* 13, 1852, *p.* 265.

85.—ON SUPPURATION IN BONE.

By Henry Lee, Esq., Surgeon to the Lock Hospital, Assistant-Surgeon to King's College Hospital, &c.

In every healthy inflammation the process of adhesion precedes that of suppuration, and pus when formed is consequently limited and circumscribed by lymph previously effused; but in unhealthy constitutions, the requisite power may be wanting to carry out the process of adhesion; and should suppuration then take place, the purulent fluid may permeate from cell to cell in the surrounding parts. These two processes are exemplified on the surface of the body by a common abscess and a diffuse cellular inflammation. In the cancellous structure of bone, the actions are strictly analogous, though somewhat more tardy in their development. The products of the inflammation may be limited by the effusion of bony matter, which fills up and obliterates the surrounding cancelli; or the secretions of the part (when the adhesive process is imperfectly developed) may infiltrate the structure of the bone to an unlimited extent. We have thus a natural division of the cases of suppuration in bone into those which are circumscribed, and those which are not; into cases of abscess, properly speaking, and into those of purulent infiltration.

These two classes of cases differ in their causes, progress, and termination. The simple abscess usually originates in young and healthy persons: the infiltration of purulent matter rarely takes place without some present depressing influence, or some former cause of constitutional weakness. The simple abscess is marked in

its progress by excessive pain, and may continue in much the same condition for many months, or even years. Diffuse suppuration, on the other hand, may be attended with little local suffering, but very soon becomes the cause of much general excitement, and leads to the formation of disease in other parts. The termination of circumscribed abscess is generally favourable, however long it may have lasted, provided the matter be evacuated externally: whereas purulent infiltration in bone is usually fatal.

Chronic abscesses require moreover to be carefully distinguished from cases of softened tubercle, with which they appear to have been confounded. In the excellent memoir of M. Nélaton, published in Paris, in 1837, we find the following description of encysted tubercle in the extremity of long bones. "When an encysted tubercle is developed in the extremity of a long bone, it is at first confined in the centre of the cancellous structure, not far from the articular extremity. It gradually increases, and approaches on one hand the cartilage, and on the other the circumference of the bone, external to the joint. If in the progress of its development it reaches the exterior of the bone, it escapes into the surrounding cellular tissue: an abscess is there formed, which increases and empties itself externally, leaving a fistulous communication with the interior of the bone. But if, on the contrary, it makes its way towards the articular surface, the cartilage with which it comes in contact is perforated, and the tubercular matter empties itself into the joint." M. Nélaton says that he has seen several examples of this unfavourable termination of the disease; and his observations are narrated with so much circumstantial detail, that we cannot doubt that he has actually traced the processes which he describes, and has related that which he himself has witnessed. But the cases of crude tubercle in adult bone to be found in our museums are so rare, we cannot but think that the frequency of its occurrence has been greatly exaggerated, and that M. Nélaton, and other continental writers, have included cases of softened tubercle and chronic abscess under one common description.

Chronic abscess may probably occur in any bone of the body. In the museum of the College of Surgeons there is a specimen where it had taken place in the clavicle; and Mr. Arnott has mentioned an instance of its having occurred in the femur. At King's College Hospital a case lately presented itself, where a circumscribed abscess had formed in the lower jaw, the bone around being greatly condensed and thickened. By far the majority of cases of chronic abscess, however, occur in the tibia, and almost always in the upper or lower extremity of that bone.

When an abscess is formed in bone, important changes occur in the surrounding parts. The periosteum and adjacent bone become inflamed and thickened. This is followed by the formation of new bone, both in the cancellous structure, and on the surface of the original bone. The bony matter thus secreted corresponds to the deposition of lymph around an abscess in soft parts. That which is formed on the surface of the bone is readily recognised. It is disposed in layers, or in small irregular masses, and never presents the fibrous structure of the original bone. This deposition may go on, as the bone in immediate contact with the pus is absorbed; so that, although the abscess is gradually making its way externally, it remains covered with the same thickness of bone as before.

Deposition of new bony matter occurs much more readily around the spongy extremities than upon the compact shafts of bone. In the former situation, it may take place to such an extent, as to render it very difficult to say precisely what part of the bone was originally affected, and a trephine applied under these circumstances may fail to reach the seat of the disease.

After a circumscribed abscess is formed in bone, the parts around appear to accommodate themselves to its presence. This is accomplished, not by the fibres of the bone being pressed asunder, but by an actual absorption of the osseous substance. The intense pain experienced depends, doubtless, upon the extreme tension mantained upon the unyielding structure, and every fresh accession of pain results from a fresh secretion of fluid. The influence of some medicines in restraining the symptoms may probably depend upon their power of promoting the absorption of the more fluid part of the abscess. Thus we find that the iodide of potassium, given in doses of three or four grains, will sometimes be followed by a temporary abatement of the pain; and it would appear from the cases recorded, *that, after the first* attack of pain, the symptoms may remain in abeyance for almost

an unlimited period, until some accidental cause produces a fresh secretion of fluid. The cavity of the abscess then again becomes distended, and the pain of compression returns. The pressure of the fluid operates on all sides equally, and tends to cause absorption in every direction. But this is counteracted by the constant tendency to the deposition of new bone. A process analogous to the pointing of an abscess in soft parts not unfrequently takes place; the ulceration affects one point of the walls of the abscess particularly, and an opening may thus be formed, through which its contents are evacuated externally.

In other cases the process of deposition goes on in the whole circumference of the bone as rapidly as that of absorption, and the abscess cannot then make its way externally. The compact structure of the shaft of the bone also prevents its extending in that direction; the articular surface is then the only one towards which the abscess can extend. No fresh layers of bone can here be deposited, and the fluid consequently makes its way towards the joint.

The cartilage has been observed in such cases to be affected in two ways: either perforated, so as to allow the matter to escape directly into the articulation, or absorbed over a large surface without suppuration. In a case of the latter kind, recorded by Sir B. Brodie, the cartilage covering the head of the tibia in some places was perfect in its structure, but it existed only in narrow stripes; in other parts it had degenerated into a substance something like condensed cellular membrane; in others, the only vestige of it was a kind of membrane, so thin, that the bone could be seen through it; and in other parts, the surface of the tibia was completely exposed, but not carious.—*London Journal of Medicine, Jan.*, 1852, *p.* 7.

86.—MEDULLARY DISEASE OF THE SKULL.

Under the care of E. Stanley, Esq., at St. Bartholomew's Hospital.

[There are two facts of an interesting character in the following case:—first, it is important to know, whether ill treatment, followed at a distant period by serious disease, should not be liable to be punished by certain penalties; and, secondly, it is an instance of the development of encephaloid disease in a locality very rarely thus affected.]

William B., aged fifteen, a farmer's boy, of interesting features, and whose family is healthy, was admitted Oct. 15, 1851, under the care of Mr. Stanley. The patient says that about three years before admission, when in service, he used to be treated very roughly by a waggoner under whom he was placed. This man was in the habit of striking the poor boy on the head with the handle of the pitchfork or the butt end of the whip. This shameful treatment was continued for nearly nine months, when one day, being struck a more than usually severe blow, which stunned him at the time, he fled from the farm and his torturer. It should be added, to complete the chain of brutal deeds, that the unfortunate sufferer was more than once taken by the heels, held with the head downwards, and the latter brought violently in contact with the ground.

About a month after the patient left his master's service, a small lump appeared at the top of his head: this tumour gradually increased for the space of two years, but it had grown somewhat more rapidly within the last twelvemonth, during which time it assumed a purple colour. The patient was treated, at the onset of the affection, in a provincial hospital, where by various applications the swelling diminished to a small extent; but this improvement was of short duration, and the tumour became afterwards so large that the boy came to town.

On admission, the head presented the following appearances:—a large, firm, somewhat oblong tumour (situated in great part above the ears) extends upwards and backwards, looking like an additional skull-cap placed upon the vertex; the scalp appears much thickened, and the growth beneath it feels as hard as bone. The occipital protuberance is very prominent, the superior portion of the scalp of a blue colour, and covered with a thick yellow crust, from whence a thick scanty discharge is issuing. The occipital and temporal arteries are very large and beat strongly; the veins of the scalp are turgid and of great size. At its widest circumference the head measures twenty-one inches and a half, and across from the

upper part of one ear to the other, sixteen inches and a half. The tumour, by rising vertically from the vertex to the size of a Dutch cheese, produces a deformity of a peculiar character, as the distance from the eyebrows either to the chin or the top of the head has become the same. The boy complains of severe pain in the forehead and eyes, more especially the right, and for the last fortnight he has experienced much uneasiness in the head when he attempted to lie down.

Within the last five weeks the patient has lost the sight of the right eye, being only able to recognise the light with it. The vision of the left eye is likewise impaired, and there is an aspect of heaviness in both organs. The pupils (which are always somewhat dilated) act sluggishly to the stimulus of light; the boy's intellect is quite sound, but his gait is unsteady, and he walks as if his head were too heavy for him. Pulse 80, soft; tongue clean and moist; bowels regular; appetite good.

After the patient had rested a few days, Mr. Stanley ordered the scalp to be shaved, and a poultice applied over the tumour. Internally the boy was given three grains of iodide of potassium, in decoction of sarsaparilla, three times daily.

The pain in the head fluctuated until the sixteenth day after admission, when about noon the patient became much excited, and complained of great suffering in the tumour. The house-surgeon applied eight leeches to the head, and gave the boy a mercurial purge. The pain being mostly situated behind the left ear, some more leeches were placed on that region a few days afterwards; and mercury with chalk was ordered to be taken night and morning. Three weeks after admission (the pain in the head having persisted all the while) the patient lost the sight of the left eye also; but the pupils continued to act slightly. Great weakness now came on, the poor boy was always crying with the agony in his head, and on the thirty-eighth day he became suddenly deaf.

An unhealthy suppuration was at the same time going on in the scalp, and an oblong excavation, five inches in length, and three in breadth, formed on the top of the head, the secretion from the same being very fœtid. On the forty-third day, the patient was seized with a convulsive fit, in which there was loss of consciousness and distortion of features. After recovering from this he became rapidly weaker, sank in the space of a few hours, and died six weeks after admission.

The inspection only included the head. On cutting through the scalp (which was not thickened) and reflecting it, a morbid growth immediately came into view, to which the scalp was adherent, the veins being empty and small. The skull was now horizontally sawn through just above the orbits, and attempted to be removed, but as it was found to be adherent above, a vertical cut was made through the tumour, skull, and brain. On examining the two surfaces resulting from the section, the disease proved to be a malignant growth which sprang from both tables of the skull, the bone being thickened at the limits of the disease, and expanded (without any perforation) at the centre of the tumour. That portion of the disease situated externally was about three inches thick at its widest part, of a whitish colour, firm feel, and closely connected with the bone. The growth from the inner table (about two inches in depth) projected into the substance of the hemispheres, and differed from that portion which was connected with the outer table both in colour and consistence; being very soft and reddish. The dura mater was involved in the disease, and the longitudinal sinus completely obliterated; the substance of the brain surrounding the disease, as well as the remainder, was perfectly healthy. The slough of the scalp extended to the surface of the growth, and it was perceived in sawing the bones that they were very thin and soft.

It would thus appear, that in this case the growth of an encephaloid tumour was excited by frequently-repeated blows upon the skull; and it may justly be asked, whether such violence may produce the disease in a previously healthy subject, or whether malignant germs *must* have existed in the patient, to allow of the development of the medullary tumour. The latter supposition would accord best with the generally adopted notions; but it will be allowed that the exciting cause in Mr. Stanley's patient is of a more important nature than is the case in the generality of malignant growths. In the history of most tumours of that kind, a slight blow or pressure is commonly mentioned as having given rise to the disease; but *here we have a* continuance of harsh treatment, and this circumstance will probably

contribute in making pathologists pay more attention to the exciting cause, especially as the subject of the preceding case was extremely young.

If the perpetrator of these brutal acts were brought before a court of justice, a question, perhaps somewhat puzzling to our medical jurists, and having a direct bearing on the pathological importance of the case, might be raised—viz., whether the mortal affection of which the boy fell a victim was or was not wholly and solely to be attributed to the ill treatment which he had endured.

The present case should likewise be viewed with respect to the phenomena which are generally supposed to arise from pressure on the cerebral mass. We find that that pressure was principally borne by the hemispheres, and the symptoms were amaurosis, deafness, and only one convulsive fit. We must therefore suppose that the optic nerves and the origin of the seventh were interfered with, though the pressure was mostly exerted on the convexity of the hemispheres. This might of course be explained by the congestion to which the pressure must inevitably give rise; but we would venture to remark, that with the fact of such compression exerted on the brain, either paralytic, or at least epileptic, symptoms, might, according to received notions, have been expected; these, however, were almost completely absent. These facts should be borne in mind by those who have endeavoured to explain the intermittent phenomena of epilepsy by intermittent pressure of some point of the cerebral mass, and who think that the disease depends more frequently on tumours acting on the *external* than on the *internal* surface of the cerebral matter. We must not conclude without mentioning that the disease in the present case was suspected to consist in the expansion of the diploë, as this pathological change had been observed before, on the autopsy of a subject somewhat similarly affected.—*Lancet, March*, 6, 1852, *p.* 238.

ORGANS OF CIRCULATION.

87.—POPLITEAL ANEURISM TREATED BY COMPRESSION.

By Dr. O'Bryen Bellingham, Surgeon to St. Vincent's Hospital, Dublin, &c.

[Dr. Bellingham remarks that when compression first came into notice it was objected that the disease would be liable to return, but in all the cases coming under his notice the cure had been permanent, and the patients in all had gained the perfect use of the limb. He then goes on to say]

From the manner in which compression brings about the cure of aneurism, it is clear that the disease cannot return in the same part of the vessel, and, in the opportunities which I have had of examining the parts some time subsequently, owing to the patient's death from other causes, the artery has invariably been found to be obliterated at the seat of the aneurism, and converted into an impervious, ligamentous band at the part; while the portion of the artery upon which pressure had been made, preserved its normal condition, and neither it nor the vein (which, from its proximity to, and close connection with the artery, must always, in cases of femoral or popliteal aneurism, be compressed with the artery) presented, in a single instance, any deviation from the normal and healthy state.

When we contrast the results of the treatment of aneurism by compression, with the results of the ligature in aneurism of the same arteries, as furnished by the most recent statistical tables, the comparison appears very favourable to compression: thus, the table which I have given contains 36 cases; in 29 a cure was effected by compression; of the remaining 7 cases the artery was tied in 2, the patients recovering. In 1, pressure was discontinued, the aneurism subsequently diminished in size, and the patient had the perfect use of the limb for three years, when symptoms of aneurism of the aorta supervened, and compelled him to give up his employment. In 2, the limb was amputated, the patients recovering, and in the remaining 2, death occurred, in one from pulmonary disease, in the other from a severe attack of erysipelas; but in both the local disease was very nearly cured, the aneurismal sacs being almost completely filled by fibrine deposited in concentric layers.

If we now turn to the statistics of the ligature, furnished by the most recent

statistical tables, the results appear much less satisfactory. Thus, in a table given by Dr. Norris in the 'American Journal of Medical Science,' 188 cases where the femoral artery was tied for aneurism are reported, (in 155 for popliteal aneurism,) and the following are the results:—

No. of cases.	Cured.	Died.
188	142	46

The majority of the deaths was owing to gangrene, secondary hemorrhage, phlebitis, tetanus, &c.; while six of the patients who recovered underwent amputation of the limb.

Another table is contained in Mr. Crisp's 'Treatise on the Disease of the blood-vessels.' This includes 119 cases of popliteal aneurism treated by the ligature, with the following results:—

No. of Cases.	Cured.	Died.
119	103	16

The deaths were the result of secondary hemorrhage, gangrene, phlebitis, tetanus, delirium tremens, the shock of the operation, and suppuration of the sac. Six of the patients under the head "*cured*," underwent amputation of the limb after the ligature; and six others recovered, notwithstanding the occurrence of gangrene, secondary hemorrhage, or suppuration in the sac.

The summary of the results of the ligature, in the cases of popliteal and femoral aneurism, contained in these tables would appear to be even too favourable to the ligature. The same cases are included in both; and it is evident that they can form only a moiety of the cases of aneurism of these vessels, in which the ligature has been used, during the period in question, (fully half a century.) When we consider, in addition, with what unwillingness cases which turn out unfavourably are published, it seems highly probable, that, if we were in possession of full statistics of all the operations for aneurism performed upon these vessels, the per centage of deaths would be found to be much greater. Even taking the statistics of the ligature as we find them, if we contrast its results with that of compression in the same forms of aneurism, we perceive a remarkable immunity from risk, in the one method of treatment, and a considerable amount of risk attending the operation in the other. Thus, the great majority of the deaths after the ligature were owing to secondary hemorrhage, gangrene, phlebitis, tetanus, and delirium tremens, in other words, directly to the result of the operation; while in the treatment by compression, all those accidents were avoided, not one of them having occurred in the thirty-six cases contained in my table. The two deaths in it were, I may say, from causes independent of the treatment; the one having been from erysipelas, which was prevalent in the hospital at the time; the other from pulmonary disease. Both were likewise men of broken-down constitution, with diseased heart and arteries; very unfavourable subjects, consequently, for operation, and in whom, if the ligature had been used, it would most probably have proved a failure.

Although compression has proved eminently successful in Dublin, as a mode of treating certain forms of aneurism, and has so completely superseded the ligature there, that the latter has not been had recourse to for several years past in a single case of popliteal aneurism, either in hospital or in private practice, it must be confessed that the reports from most other places are less favourable; it is therefore scarcely unreasonable to conclude that compression cannot have been employed in the same way, that equally effective instruments cannot have been used, or that the same precautions cannot have been taken to ensure success. Indeed, a perusal of the cases which have been published of the failure of compression in the hands of surgeons out of Dublin (which have been put so prominently forward in some places) has satisfied me, that the failure in the majority resulted either from the imperfection of the instruments employed, from an unnecessary degree of pressure having been used, or from its having been made in an imperfect manner, from tight bandages having been applied to the limb while pressure was maintained upon the main artery, from the treatment having been commenced without due attention to constitutional measures, or finally from compression having been used in cases where the ligature would equally have failed.

It is unnecessary to say that compression is not employed at the present day with the object of endeavouring to obliterate the artery at the point compressed; if it were, *few patients would have* been found possessed of the fortitude to submit to the

pain such an amount of pressure would occasion. I do not, however, mean to assert, that compression is a painless process; but I do say that the pain may be much increased, and that the patient may be, and often has been, put to a great deal of unnecessary pain, and may refuse to continue its use from this cause alone.

I do not wish it, either, to be supposed that compression is advocated in every form of aneurism, or that it is supposed to be capable of effecting a cure in every variety of the disease; as, for instance, in diffused aneurism, or where the aneurismal sac had been allowed to attain an extremely large size, or where the sac of a popliteal aneurism had formed a communication with the knee-joint, or had caused erosion of the bone; in fact in cases where amputation is perhaps our only resource.

Compression, as a mode of treatment in aneurism, is advocated mainly as a substitute for the ligature, and for cases to which the ligature is applicable, provided the sac is so situated that compression can be made upon the artery at its cardiac side, as well as for cases where the arterial tunics are diseased, and the ligature would be consequently very likely to fail. It is advocated because pathology has shown that it brings about the cure of aneurism by the very mode in which nature, under the most favourable circumstances, effects this object, and because experience has proved it to be a safe and certain method of treatment, while all experience has shown the ligature to be directly the reverse.

If we expect, however, to be successful, and if we hope to effect a cure speedily, our efforts must be mainly directed to the means whereby pressure may be maintained with as little inconvenience as possible to the patient, which will depend in a great measure upon the kind of apparatus used, and its completeness; upon the manner in which the pressure is maintained; and, upon the constitutional measures adopted previous to the treatment being commenced, and while it is in progress.

A good deal will also depend upon the intelligence and tractability of the patient. In a mode of treatment which usually occupies several days, and sometimes more, it is not easy to have a constant surveillance; and if the patient unscrews the instrument the moment the surgeon's back is turned, the cure must of course be retarded; or, if he is too stupid to understand the manner of using the compressing force, or the object with which it is applied, the pressure will be imperfectly maintained, and at each visit the surgeon will probably find the pad of the instrument resting upon any other part than the artery.

With respect to the instruments, I need only observe that those which are available now, are in many respects superior to those we were in possession of a few years ago, the unyielding force of the screw has been superseded by the elastic force of the vulcanised India-rubber bands, an improvement for which we are indebted to Dr. Carte; the treatment, consequently, has been much simplified, and the pressure can be borne with less inconvenience by the patient. While upon this point I may observe that it is scarcely doing justice to this method of treating aneurism, to undertake it without being provided with a proper apparatus for carrying it out; yet this would appear not unfrequently to have occurred in the trials of compression made in other places, and these very cases have been afterwards published as examples of the failure of compression, and have been made a ground for decrying this method of treatment, while the fault lay, in a great measure, with the surgeon who commenced the treatment without having proper instruments at hand for carrying it out.—*Medico-Chirurgical Transactions*, vol. xxxiv., 1851, *p.* 153.

88.—*Novel Treatment of Aneurism.*—[Professor Fergusson, in a recent case, adopted the following novel and ingenious method of treatment.—]

In imitation of an occurrence which occasionally happens by accident in cases of aneurism, viz: displacement of the mass of fibrine, or a portion of it, which is usually present in such tumours, whereby, in consequence of alteration in the current of the blood, a spontaneous cure results, Mr. Fergusson has, by manipulation of the tumour, thrown loose a portion of the fibrine in the case alluded to, with the effect of instantaneously arresting all pulsation in the upper limb. In four days a feeble pulsation at the wrist could be detected, but the axillary has been pulseless since. The tumour itself, which was at first about the size of a small hen's-egg, has diminished considerably, and the throbbing within is now little greater than in

the subclavian artery of the opposite side, while it has become more solid to the touch. To those familiar with the pathology and treatment of aneurism, and especially the fatal results which have hitherto followed all attempts at cure by operation on the subclavian on the tracheal side of the scaleni muscles we need hardly point out the interesting character of the case now under Mr. Fergusson's care.—*Med. Times and Gazette, March* 6, 1852, *p.* 255.

89.—*Treatment of Varicose Veins by Needles and Sutures.*—It is an established rule in surgery that as long as varicose veins of the lower extremity, or varicocele, do not create much inconvenience, this abnormal state of vessels should not be interfered with; but when the dilated veins become very troublesome, or threaten to give way, it is urgent that relief should be afforded. Our readers are aware that Mr. Mackmurdo, at St. Thomas's Hospital, has been very successful in such cases with potassa fusa, and Mr. Gay, at the Royal Free Hospital, has used the sutures with favourable results. Mr. Fergusson, on the other hand, has operated with much success on various patients with the needles, and sutures, in cases of varicose veins of the lower extremity; and as some surgeons are rather timid on this subject, we shall just mention the following case.

Hannah G——, twenty-six years of age, a servant, has always enjoyed good health, but having had much standing work in her situation, and many pairs of stairs to ascend in the day, has lately found the veins of her leg enlarging considerably. The patient has worn bandages and elastic stockings, but with little relief; and when admitted on July 24th, 1851, the pain and inconvenience had much increased, the left saphena vein being large, tense, and tortuous.

On the 28th, Mr. Fergusson transfixed the vein with needles, and applied the twisted suture, as in hare-lip cases; the patient was enjoined perfect rest; some pain and tenderness arose in the vein; this was, however, soon subdued by fomentations, and the patient was discharged in a few weeks with complete obliteration of the vessel. Mr. Fergusson has now in his wards a patient affected with varicocele, who has been operated on pretty much in the same manner as in the above case.—*Lancet, Dec.* 20, 1851, *p.* 580.

90.—*Case of large Subcutaneous Nævus Cured by Vaccination.* By John Woolcott, Esq., Surgeon to the Kent Ophthalmic Hospital.—[The child in this case was first presented to Mr. Woolcott's notice in January, 1848. The nævus, which was very extensive and of a blue, livid colour, was situated around the right eye, extending outwards as far as the root of the ear, and anteriorly covering the malar prominence.]

The treatment for the first month consisted in the application of tincture of iodine; the abnormal growth being freely punctured all over with a fine cataract needle, and the iodine applied over the punctures. The bleeding was considerable, and of arterial character, but it soon subsided on the application of the iodine. These punctures were made twice a week, but the iodine was applied daily, except when it caused too great irritation and soreness of the skin, when it was discontinued for a day or two, and then resumed. At the end of the month, the disease remaining undiminished, I altered the treatment and applied vaccine lymph: with a lancet armed with the matter, punctures were made at short intervals all around the circumference of the tumor, and several points in the centre of it; to ensure its taking, I inserted into each puncture a bone-point, also well armed with vaccine lymph; most of these punctures took, and the irritation they caused was considerable, the child's face and head being swollen enormously. This was attended with fever and much constitutional disturbance, but at the end of a fortnight it had somewhat abated, and at the end of a month the disease was evidently decreasing; at the expiration of six months from the vaccine lymph having been used, not the least swelling existed, and the skin was assuming its natural colour. I saw the child the beginning of January, 1852, and not a vestige of the morbid structure remains; and it was only by looking closely for the vaccination scars, that I could

tell on which side the nævus had been. I have treated several cases in the same way at the Kent Ophthalmic Hospital, and have succeeded in arresting their growth, but I have never seen so large an erectile tumour cured bythis treatment, nor can I remember to have read any such case. The colour of this vascular tumour was venous, the bleeding was arterial.—*Lancet, March* 13, 1852, *p.* 261.

91.—*Removal of a Nævus by Platinum Wire, heated by a Galvanic Current.* By J. Hilton, Esq. F.R.S., at Guy's Hospital.—[Cases of fistula in ano and hæmorrhoids having been successfully cured in this manner, in University College Hospital:]

Mr. Hilton has been trying this plan of cutting and searing at the same time upon a nævus of the flat kind, situated in front of the ear of a child two months old. The operation was performed with Cruikshank's battery and a very thin wire, which was first intended to tie around half the tumour, which was about the size of a crown piece. But the wire ran so easily through it, that the whole was completely removed, and the parts are now fast cicatrizing. This is rather a quicker measure than the ligature, and just as secure, since hemorrhage is so rare.—*Lancet, Jan.* 31, 1852, *p.* 120.

92.—LARGE PULSATING NÆVUS OF THE SCALP,

REMOVED BY LIGATURE, AND STRANGULATED IN THREE PORTIONS.

Under the care of T. B. Curling, Esq., F. R. S., at the London Hospital.

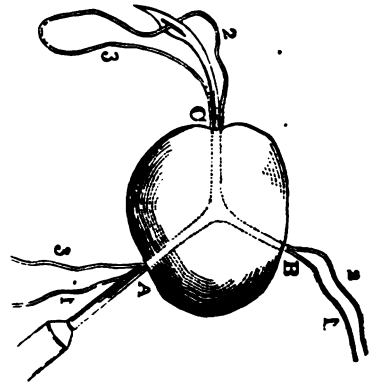
[In the later volumes of 'The Retrospect' have been recorded many cases illustrating the different and ingenious methods of applying ligatures to nævi, in such a manner as might ensure the complete constriction of the base of the growth, and its consequent sloughing away. The following is the method which has been successfully adopted by Mr. Curling:]

Edward W——, aged nineteen, from Brompton, in Kent, was admitted into the London Hospital, June 17, 1851, under the care of Mr. Curling, on account of a large, pulsating nævus on the head. A slight discoloration and swelling of the scalp were first observed, when the patient was a child, after a fall; but these appearances had remained stationary until the present period. About a week before his admission, observing some considerable enlargement, he was induced to apply to a surgeon in his neighbourhood, who, finding the nævus rapidly increasing, sent him to London, for admission into the hospital.

On examination, there was found a soft, compressible swelling, of large size, at the upper and back part of the head, towards the right side. The swelling pulsated distinctly, and was fed by some large and tortuous branches of the occipital and temporal arteries. The puffy swelling extended for some distance in the course of two of these vessels, and formed apparently prolongation of the nerves. The skin over the prominent part of the tumour exhibited several scarlet spots, but the disease was chiefly subcutaneous.

On the ninth day after admission, the patient having been narcotized by chloroform, Mr. Curling took a rather large needle, fixed in a handle with the eye near the point, armed with a long and strong ligature of twine, and passed it through the base of the tumour, so as to isolate about one-third; the loop being divided so as to leave one ligature free and of proper length, the needle retaining the other was withdrawn as far as the centre of the swelling, and then carried through the base of the larger part of the same, emerging at its circumference midway between the points of entry and exit of the needle. A second loop was drawn out to a sufficient length and divided, and the ends freed from the needle, which was then withdrawn: thus leaving three separate ligatures, which were in contact at a point in the centre of a tumour, and divided it into three equal parts. The method will be at once understood by a glance at the annexed sketch. The skin was next incised round the circumference of the tumour, between the three points at which the ligatures emerged. Arterial blood gushed out freely at these points, as well

as from the incisions, but the bleeding was readily arrested by pressure. The ligatures were tied as firmly as possible, encircling the base of the nævus, and strangulating it in three segments, when all bleeding ceased.



A. Point at which the needle was introduced. *B*. Point at which the needle first emerged, and from which, after division of the loop, it was withdrawn as far as the centre of the nævus and carried on to *C*. The dotted lines mark the course of the ligatures through the base of the nævus. The figures 1 1, 2 2, 3 3, point out the continuity of the three threads.

No inflammation or irritation about the scalp followed the operation. On the fourth day, in order to hasten the separation, Mr. Curling applied a single silk ligature tightly round the base. The large strangulated mass separated on the fifteenth day after the application of the ligature, and in a day or two afterwards the sore assumed a healthy granulating surface. This healed slowly, and the patient was discharged forty-two days after the operation, with the wound nearly closed.

The puffiness and enlargement of the vessels leading to the tumour, as well as all pulsation which existed beyond the boundaries of the ligature before the operation, had quite disappeared, and the scalp presented its usual appearance.

The preceding case not only illustrates a very effective manner of employing the threads for the constriction of nævi, but it likewise shows that the fear of hemorrhage in operations upon pulsating erectile tumours, may now and then have been somewhat exaggerated. Caution is certainly a good quality, but it should not degenerate into a puerile timidity. Great loss of blood might certainly have been expected with this patient; but the ease with which pressure commanded the hemorrhage will show that we may safely be a little bolder, in similar circumstances, than is usual with the majority of surgeons. As long as plunges with the knife are avoided, and the ligature carefully applied, there need not be much apprehension,—provided always the tumour be not too large, or situated in an exceptionably vascular region.

Being on the subject of nævi, we should not omit to mention, that we have seen Mr. Lloyd operate very frequently upon such abnormal growths, at St. Bartholomew's Hospital. Mr. Lloyd destroys nævi slightly raised above the surface with potassa fusa, and there is hardly ever a return of the growth. We have seen him use the caustic upon very young children, and the latter did not seem to suffer much pain. With erectile tumours, Mr. Lloyd excites the sloughing action, by injecting into the substance of the growth the aromatic spirit of ammonia, by means of a small syringe, with a long and delicate pipe. The nævi seldom resist the action of the caustic fluid, and generally become obliterated in a few weeks.—*Lancet, Jan.* 17, 1852, *p.* 74.

93.—Simple Cupping Instrument.—At a recent meeting of the Suffolk District

Medical Society, Dr. Gould exhibited a new instrument for cupping, formed of a thick, firm, hollow, India-rubber ball cut in half. In applying it, all that is necessary is to place the concave surface on the part that is to be cupped, and press down the centre to exhaust the air, after which the fingers may be taken off, and the ball will be found to adhere—and quite firmly, too—by means of the vacuum created. For all the ordinary purposes of cupping, this simple contrivance will be found most effectual, and particularly in dry cupping. It has advantages over glass cups, either with or without the pump, and the cost of a dozen of them would not exceed a dollar; besides, there can be no breakage, or liability to get out of order, nor is anything required to exhaust the air from them, save the pressure spoken of when they are applied. To the country practitioner, who cannot always have leeches at his command, this simple and cheap substitute will be most acceptable.—*Boston Med. and Surg. Journal.—Med. Times and Gazette, March* 13, 1852, *p.* 270.

ORGANS OF RESPIRATION.

94.—ON TRACHEOTOMY.

By William Fergusson, Esq., F.R.S., &c.

[If there is a necessity for enlarging the wound in tracheotomy to introduce the canula, the knife should be carried upwards, and not below. The reason of this is, that]

There is always much less danger in carrying your incisions upward, in this operation, than in cutting at the lower part of the wound; for, if you happen to get very low down, you might come in contact with important blood-vessels; the arteria innominata, in some cases, rises very high, and that might be wounded; but more especially might the vena innominata be in the way. At the upper part of the wound you may cut without much fear; it is true, there is the isthmus of the thyroid body in the way; but this does not matter much, so that you keep exactly in the median line. You must not misunderstand me, and fancy that I would wish you to cut through the isthmus. You ought, if possible, in opening the trachea, to get below this process on all occasions; but it is not easy or judicious to do so in many instances. In the case under notice, the neck was very short, and I was obliged to divide the isthmus of the thyroid body. It is most important to recollect these two points in performing the operation of tracheotomy: in the first place, not to use your knife freely at the lower part of the wound; and to take especial care not to deviate from the middle line.

You must have observed that there was a good deal of bleeding when I made the first incisions; and, therefore, although the patient was in imminent danger of dying from suffocation, I was obliged to wait a moment or two before I opened the trachea. This step of the operation was by no means an easy task; for the convulsive movements of the throat were such as to prevent me from placing my finger on any one point of the tube; and you saw I was obliged to open the windpipe by a sort of jerk of the knife. When I introduced the tube, there was a little arterial blood forced out of it. This bleeding was from the mucous membrane of the trachea, while that which was noticeable before was mainly or entirely venous. You must also have noticed how very little irritation the introduction of the tube into the trachea caused. We read in books, and hear from those who have not had much experience in these cases, of the very great irritability of the mucous membrane of the trachea; but this is a mistake; the inside of the trachea is not very irritable. This irritability, which is so marked in the passage to the breathing apparatus, is confined to the upper part of the windpipe, namely, to the larynx, and vocal chords.—*Med. Times, Nov.* 8, 1851, *p.* 480.

95—FUNGOID TUMOUR OF THE CHEST.

By Henry Smith, Esq., formerly House Surgeon to King's College Hospital.

[It was somewhat remarkable in this case, that although the tumour to be described

was apparently of a malignant character, yet during the three months the patient had been in the hospital, she had improved considerably in her general health.]

The following are some particulars of this curious case:—Mary Ann Scanlan, aged 14, was admitted into King's College Hospital, Nov. 10th, with a large swelling situated in front of the left axillary space. She has lived in Greenwich. Her mother is quite healthy; but her father died of what appears to have been a malignant growth in the groin, which was removed, but subsequently returned and caused his death. About two months ago the patient observed a small lump in the left axilla, which increased very rapidly up to the present time, particularly during the last five weeks. When she first observed the swelling, she was in good health; but since that time she has been losing flesh rapidly. On admission, she was wretchedly emaciated and very pallid, thus causing the tumour to stand out in bold relief; this being a large, smooth, but hardened mass of a conical shape, and considerably larger than the two fists, occupying the whole of the left axilla, and extending backwards as far as the under surface of the scapula, to which it appears to be attached; anteriorly it projects from the axilla to within an inch and a half, or two inches of the sternum, reaching as high as the collar-bone, and nearly as low down as the nipple of the left breast. It was quite immoveable, and had a somewhat elastic feel. It had the usual appearance of a fungoid tumour, and the enlarged veins over it were conspicuous. The pain was excessive when it was handled; and indeed there was a persistence of a severe lancinating pain at all times in the tumour, more especially during the day. The surgeon in attendance upon her had, a few days prior to her admission, made a puncture into the tumour, and evacuated nothing but blood. Mr. Fergusson made a careful examination of this case, and gave as his opinion that it was a fungoid tumour; but that, from the nature and connections of the mass, it was by no means an advisable thing to attempt any operation; and as the patient was in such a wretched condition of health, she was ordered to take good nourishment, including wine and cod-liver oil.

After she had been in the house ten days, the health of the girl had greatly improved; the tumour, however, appeared to be increasing in size, and around the spot where the puncture had been made there was considerable redness, and an appearance as though it were about to ulcerate. Cramps in the arm annoyed her. The sensation of the arm and hand was much modified, in consequence probably of the pressure of the tumour on the axillary nerves.

On the 13th of December one of the dark patches on the surface of the tumour gave way, and a large quantity of blood escaped,—the nurse thinking that at least a pint came away. The house-surgeon was sent for, and applied gallic acid, which entirely restrained the bleeding. She did not seem to have suffered much from this loss of blood. On the contrary, her improvement, which had before commenced, steadily progressed,—she had become plumper, and got excellent spirits,—the tumour decreased in size, and, although the bleeding had occurred on other occasions, the girl, who is at present in the hospital, is in a much better condition of health than when she first came in, although there is a profuse discharge from the tumour.

At the latter end of January the disease put on a different aspect. From the breach of surface which already existed, an offensive, sanguineo-purulent discharge took place, and the tumour evidently lessened in size. The patient herself suffered considerably more from constitutional disturbance, and there was a great deal of pain in the situation of the disease. Poultices were applied to the tumour.

Feb. 4. Still continues very ill. She refuses to take her food. The discharge from the tumour is exceedingly fetid, of dark colour, and in great quantity, saturating everything that is near the tumour.

Feb. 7. This day a fungous-looking mass protruded through the opening at the posterior part of the tumour, of a dark reddish appearance, and, indeed, on close examination, looking very much like a mass of fibrine. The substance continued to protrude, and on the 9th it had become so much detached from the tumour that Mr. Fergusson removed it without difficulty by means of a pair of forceps. On examination, it appeared to consist of fibrin. Mr. Fergusson made *the remark, that it was* very similar to the laminated mass which is found inside an

aneurismal sac, and now his opinion was entirely changed as to the tumour having been malignant in its nature.

On Feb. 12, while the nurse was washing the diseased parts previous to applying another poultice, a large mass of the same nature as was before removed came away, leaving a large wound from which it had escaped. The mass was larger than an egg, irregular in shape, flaky, and capable of being torn into laminæ. On a section being made, there were found to be cells of considerable size, which were filled with a thick dark mass, somewhat resembling half-coagulated blood. Dr. Henry Monckton, of the College of Surgeons, examined it carefully under the microscope, and I cannot do better than give the result in his own words:—"It is no tumour, but a mass of fibrin, blood-corpuscles entangled in some parts, no other tissue being mingled with it. It would appear to have been slowly effused, being laminated, and having a somewhat fibrous appearance both to the naked eye and beneath the microscope. The effusion would appear to have been intermittent, the several parts having different degrees of consistence, and forming several masses more or less globular."

Since the last portion was discharged, there has been no appearance whatsoever of tumour, the chest has become natural in shape, and the scapula is no longer distorted from its usual position; there is a large, healthy-looking wound in the axilla, from which there is but a moderate discharge, and the girl herself is again getting into a fair condition.

It is not very often that one meets with a case which presents features of such great interest to the pathologist and to the practical surgeon; it is one, indeed, which is replete with instruction as well as interest, for it demonstrates the necessity of caution in giving a decided opinion as to the real nature of a tumour which has the appearance of being malignant. In all cases of doubt and difficulty, the surgeon should be excessively slow and diffident in making up his mind, or at all events, in letting others know his opinion as to the nature of a disease; but when it is a question as to whether a tumour is malignant or not, this necessity for caution obtains to a greater degree, inasmuch as the carrying out of some important practical measure—as a serious operation—may depend upon the opinion which is to be given. In the case in question, there can be no wonder that the disease turned out different from what it was considered to be. When the patient was first sent up to the hospital, there were all the indications of malignancy in the tumour; and, in fact, the intelligent surgeon who had sent her up (Mr. Bradley, of Greenwich) had supposed that Mr. Fergusson would consider it proper to extirpate the tumour by the knife. This gentleman and all those who saw it considered that the disease was malignant in the first instance. In some clinical observations which Mr. Fergusson made in reference to it, he stated, that he did not operate because he considered the tumour to be a malignant one, and also because the child was in such a wretched state of health that she seemed unequal to undergo an operation. After the patient had been in the house a short time, certain changes took place which made him hesitate as to his opinion of the tumour being malignant,—it began to diminish in size, and the patient herself rapidly improved in her appearance. She became comparatively so fat that he looked upon it as one of the most remarkable features connected with the case, more especially if the tumour really were malignant, for we generally find that when malignant disease is present there is gradual wasting, and no tendency to improvement in the general nutrition, even under the most favourable circumstances. When the diseased mass protruded, he considered that the substance was entirely different from the ordinary fungus which is seen in malignant tumours, but that it was more like the coagulated blood found inside an aneurism; and he then considered that the disease was not malignant, but that the swelling was formed of effused and coagulated blood. This had turned out to be the case, but how the tumour was formed it was impossible to say; whether the effusion of blood had occurred spontaneously, or whether it was the result of a blow, it could not be ascertained. He could not help remarking, how exactly the report of Dr. Monckton tallied with the facts which had occurred; and he was happy to give him the credit of being so accurate an observer. This gentleman had stated, that from what he had observed of the mass which had been removed, he had no doubt that the effusion had been intermittent. Now, he himself had noticed that the tumour had from time to time

varied in size, and no doubt it had been formed by slow and repeated effusions of blood. Mr. Fergusson, in concluding his remarks, stated that this case had been one of a most important nature, and that he hoped all those who had seen it would carry the recollection of it upon their minds. Such instances were extremely rare; he had seldom seen one like it, and it proved how necessary it is to be cautious in giving a decided opinion as to the precise nature of a tumour.—*Med. Times and Gazette, Feb.* 28, 1852, *p.* 215.

96.—*Wounds in the Diaphragm.* By G. J. GUTHRIE, Esq., F. R. S.—[It is a singular fact first noticed by Mr Guthrie, that a wound in the diaphragm never heals, but remains a hole even if the person should live for years.]

A soldier of the 12th Dragoons was wounded, at Waterloo, by a sword, which penetrated the left side of the chest diagonally, and came through the opposite or right side below, having in its way, as was supposed at the time, injured the diaphragm. The man recovered, and returned to his duty. On the 6th of September, nearly fifteen months afterwards, he was attacked, after cleaning his horse, by all the symptoms of a strangulated internal rupture, and died next day. On opening the body, the greater part of the stomach was found to have passed through a hole in the diaphragm, made by the sword, and had become strangulated, like any other part constituting a hernia or rupture.

When a man has recovered from a wound which has been supposed to have left a hole in his diaphragm, he must not clean a horse, nor tie his own shoes, nor even bend his back, beyond making a bow—I do not forbid that—if he cannot help it. He must eat sparingly at a time, drink less, and sleep as much as possible in a somewhat raised position. When, however, all possible precaution has been taken in vain, and he is distinctly suffering from the symptoms of an internal strangulated hernia of the part supposed to have been injured, and is likely to die unless relieved, what is to be done? Why, you must make an incision through the wall of his abdomen, just over the suspected spot; introduce your hand through this cut, feel with your forefinger for the hole in the diaphragm, and withdraw the strangulated part, even it should be necessary to enlarge the hole in the diaphragm by a blunt-pointed bistoury. If it should have formed attachments and cannot be withdrawn, the strangulation may be relieved and the patient survive. It is an operation you can practise every time you open a dead body, and it will be a great triumph for surgery to save the life of even one person labouring under an infirmity otherwise at this moment invariably mortal. It will be my reward for the extra labour I went through in 1811.—*Lancet, Jan.* 31, 1852, *p.* 113.

ALIMENTARY CANAL.

97.—ON CLEFT PALATE.

By WM. FERGUSSON, Esq., F. R. S., &c.

It will be recollected that Mr. Fergusson pointed out some years since that the well-known approximation, during deglutition, of the soft margins of the fissure, is owing to the contraction of the superior constrictor of the pharynx and the upper portion of the middle constrictor; and that the palato-pharyngei and levatores-palati muscles tend to separate the gap of the velum. Hence Mr. Fergusson inferred, that "if the chief muscular action affecting the soft palate could be done away with either for a short time or permanently, there would be a greater probability than ever of a union taking place in the mesial line when the parts were united by the process of staphyloraphy." As a preliminary step, therefore, to the ordinary operation, the division of the levator palati on each side was suggested, and also, if it seemed needful, of the posterior pillars of the fauces, whereby large portions of the palato-pharyngei might be cut across. Mr. Fergusson also thought that the anterior pillars, each containing the palato-glossus, might possibly require division.

[The patient in the following case was admitted into the Hospital, December 1851, with congenital fissure of the soft palate. Mr. Fergusson first generally uses a gargle of alum to allay the vascularity of the parts and astringe the tissues; also *frequently* irritates the back of the mouth with a feather so as to gradually *unish the natural* irritability of that region.]

Mr. Fergusson operated December 20th, 1851, and began by dividing the levator palati and palato-pharyngei muscles with a bistoury, the cutting portion of which forms a small triangle, and is placed at right angles with the stem. The hæmorrhage after this preliminary myotomic step was not considerable,—indeed, Mr. Fergusson has generally found it trifling, provided the incision be kept within proper bounds. The following are the operator's directions:—

"There might be imminent danger if the knife were carried far back above the palate, but so long as it is limited in its action between the posterior nares and the posterior border of the levator palati, there is no possibility of reaching any large vessel. Further back than this, there might be a risk of the point passing out at the side of the upper part of the pharynx, and doing serious mischief." The margins of the soft fissure having been pared, the threads were passed by means of a needle set in a handle, with the eye at the extremity, the threads being, as usual, of different colours.

It requires much coolness and patience,—whilst the mouth fills with blood and saliva, and the patient, perhaps, involuntarily coughs,—to seize, by means of the forceps, the thread which appears with the point of the needle in the fissure. This thread was drawn for several inches out of the mouth, and made to lie (being double, of course,) over the commissure and chin. The same is done on the other side of the gap; and the two inner ends of the four, which are hanging out of the mouth, are then tied. The outer end on the patient's left is now drawn outwards, and naturally carries the thread lying on the patient's right across the fissure through the second aperture, and finally out of the mouth. Thus was there now one thread ready for tying.

This latter process of tying, Mr. Fergusson manages in a very neat manner. He makes a loose loop with the end of the thread lying on the patient's left, the end of the other thread, on the right, is passed through the loop, and that end being gently drawn away from the patient, the loop slides inwards towards the fissure, the two margins of which it nicely and securely brings together; a common knot is afterwards added and the threads cut short.

The bystanders cannot minutely follow all the stages of the operation within the mouth, but by seeing the proceedings repeatedly, as is the case with ourselves, all the steps of it become familiar, though its difficulties grow more and more striking. Three stitches were applied in the manner above described, and the boy, who had behaved extremely well, returned to the ward.

The parts were lax and thick, and therefore favourable for the operation. The patient was kept on liquid food, though not starved, as was formerly the custom, and strictly prohibited from talking. Mr. Fergusson did not disturb the parts for eleven days; after which time, he removed the three stitches, and union was found perfect. The boy was now allowed to speak. Some difference was at once noticed in his intonation. He was discharged on the fourteenth day after his admission; and it may be presumed, that by careful practice and training, the voice will become natural.

It will be perceived that the stitches were allowed to remain for *eleven days*, and the result has certainly been very satisfactory. Formerly the threads were taken out very early; but Mr. Fergusson has now found that it is better to leave the stitches a pretty long time, for fear of breaking adhesions.—*Lancet, Jan.* 31, 1852, *p.* 118.

98.—*Case of Inflammation and Ulceration of the Throat and Tongue Successfully Treated by the Nitrate of Copper.* By Dr. Wm. Moore, A. B., Physician to the Ballymoney Dispensary, &c.

Case.—M. A., aged 20, though apparently not more than fifteen years of age, of a fair complexion and delicate figure, consulted me on the 16th December, 1850, about her throat, which had been affected since the spring of 1845. About this period she suffered from a severe attack of sore throat, which was partially got under; but a trace of it still continued through the following summer, and till the succeeding spring, when she had a repetition of an affection somewhat similar in character, but more formidable in its results. Ulceration engaged both the tonsils and palate, and led to suppuration, the matter being discharged internally.

Under these circumstances, the parents of the patient first sought medical aid. In spite of all treatment, the ulceration gradually extended, the hard palate becoming engaged. The patient was sent to the sea-side, where she remained about a month. Repeated periodic attacks of cynanche tonsillaris ensued, until the year 1848, when she abandoned all medical advice. Her voice at this date was much affected, and several spiculæ of bone came away through the anterior nares. About the spring of 1850, the patient first began to cough. The expectoration was of a frothy and slightly purulent character, particularly on awakening in the morning. At the same time, she suffered from a most profuse salivation, which became more copious towards evening, and was accompanied with a severe pain shooting to the right ear. A month previous to her consulting me, she felt occasional palpitation, which, she stated, gave her little concern. The patient had never menstruated, nor was her mother aware of any constitutional effort to bring about that change having taken place at any period. Both her parents are remarkably healthy; her father having served in the capacity of parish-clerk for many years, and never having had any delicacy of the throat or chest. This disease had no connexion whatever with any syphilitic taint. Such is the previous history of the case, which I collected in the most careful manner, when I was first consulted.

The phenomena manifest on a careful examination were as follows. The uvula and soft palate had been wholly removed. The opening of the pharynx was easily seen, the mucous membrane presenting an abraded and ulcerated appearance. The hard palate was attacked by the ulcerative process, as well as the septum narium, and a thick, tenacious slough was closely adherent to each. The epiglottis was partially destroyed; and the larynx was deeply implicated, as I inferred from the complete loss of voice. The aspect of this extensive ulceration was uneven and granular. The tongue was irregularly fissured, hard, and warty to the touch, and triangular portions of it were insulated by clefts, some of which were clean, and of a bright red colour, and others covered with a glutinous dirty-white substance. A trace of each tonsil still remained; and, from the pain affecting the right ear, it is natural to suppose that the ulcerative process had extended along the Eustachian tube. The patient suffered from slight dysphagia; and, after the slightest exertion, from a severe hoarse cough, accompanied with frothy expectoration.

[On examining the chest, a peculiar series of symptoms presented themselves, which, after careful consideration, it was concluded arose from scrofulous excrescences on the valves of the heart, more particularly on those of the aorta. For this, the iodide of iron and pil. aloes with myrrh, were ordered. On Dec. 30th, the ulcerative process was somewhat checked, though the tongue was still covered with white patchy ulcerations. On the 2nd of the following June, there were manifest symptoms of retrogression. The mist. ferri co. was ordered, with iodine, and a gargle of dilute hydrochloric acid and tincture of catechu to be used at the same time.]

As so many caustic applications had been applied, and, to a certain extent, ineffectually, I touched the throat and tongue with *nitrate of copper*, a caustic, as yet, comparatively little used, but to the incalculable benefit derived from the application of which, not only in this, but also in many similar cases, I can bear the fullest testimony. After this treatment had been persevered in for about a fortnight, the throat had become quite clean, the sloughy discharges had almost disappeared, the tongue was also clean and free from ulceration, and the general health was much improved. Being now satisfied of the good effects arising from the first application of the nitrate of copper, I determined to repeat it, continuing the gargle as before; and I prescribed at the same time a dessert-spoonful of cod-liver oil to be taken three times a day.

The results demonstrate the value of nitrate of copper as a caustic; a remedy which I have also found effectual in similar cases, where, in spite of the ordinary topical treatment, the disease had remained unchecked. Its caustic properties may be objected to as being too powerful, and its application may be alleged to be fraught with danger; but in every case in which I have used it, no bad effects had ensued, nor is there reason to apprehend any if the following precautions be attended to; viz., to dry the ulcer or part to be cauterised before applying the *nitrate, and afterwards* to smear it with oil. For further information relative to

the successful application of the escharotic, the reader may consult an excellent treatise on *Diseases of the Tongue*, by Dr. Fleming, in No. xix. of the 'Dublin Quarterly Journal.'—*London Journal of Medicine, Jan.*, 1852, *p.* 32.

99.—CASE OF STRICTURE OF THE COLON,

SUCCESSFULLY TREATED BY OPERATION AFTER THIRTY DAYS' OBSTRUCTION; WITH AN ANALYSIS OF FORTY-FOUR CASES OF ARTIFICIAL ANUS.

By Cæsar H. Hawkins, Esq., Surgeon to St. George's Hospital, &c.

[In this case, a lady, aged 44, was relieved by the performance of Amussat's operation on the descending colon, in August, 1851, and experiences so little inconvenience as to be able to enter as usual into society, the artificial anus being kept free by means of an ivory plug of proper size and length.]

The author proceeded to say, that although M. Amussat could only find six instances of artificial anus, when he brought forward his Memoirs in 1839 and 1841, the operation had been performed in almost every year since that time, and four persons were now living in London, whose lives had been saved by its performance, and therefore he thought that sufficient cases might now be brought together to show what was the real value of the operation in surgery. He had therefore framed tables of every published case with which he was acquainted, and of seven unpublished cases besides his own, for the particulars of which he was indebted to the operators. The tables were divided into those which had been operated on through the peritonæum, seventeen in number, and those in which the bowel had been opened external to the peritonæum, which amounted to twenty-seven cases; and they showed the name of the operator and the date of the operation, with references to the published accounts of each case; the sex and age of each patient; the nature of the obstruction; the part which had been opened, and the mode in which the operation was performed; and the result, with the date of the death or of the last account of the case; and finally, the cause of death, and the condition of the patient if alive. The *results* of the operations were next tabulated, from which it appeared (omitting one case in which the operation was performed for fistula) that ten had died within forty-eight hours after the operation, and twenty-one within the first five weeks; and that twenty-two only could be fairly considered as having recovered from the operation. It was next shown, that of the twenty-two which recovered, six died in about six months from the time of the operation; others were still alive, or were so at the last known date; and that only *nine* patients were as yet known to have survived as much as one year. Against this apparently unfavourable result, the author brought forward proof that, whatever the time was that the patient had survived, the life had in every case been clearly prolonged by the operation, since Mr. Luke's case was the only one in which the fæces had chiefly passed by the natural anus after the operation; in Mr. Clement's case, which lived three years, not even flatus had passed per anum; and in Mr. Maitland's case none whatever had passed naturally after the first two years, although the patient survived the operation seventeen years. A number of tables were next brought forward, but were only partially read at the Society to show how far the results might be influenced by different circumstances. The sex of the patients did not appear to have any influence; the table of ages showed the curious fact that of eight persons not exceeding thirty, who had been operated on, no less than five had died, while of seven exceeding sixty, only two had died; but nevertheless, the age exerted less influence than might be supposed from this circumstance, since the cases below forty and those above fifty, each showed nearly an equal number of deaths and of recoveries. The table of diseases for which the operation was performed, showed that no less than seventeen were believed to be cancerous; but although the immediate deaths were slightly increased by the debility of the cancer, the deaths of those that recovered were not produced at an earlier period than in non-malignant diseases. It was next shown from the tables of the assigned or apparent causes of death, both in those who recovered, and in those who died in the first five weeks, that scarcely any died of the operation, but that organic

changes or other effects of the disease itself could in almost every case be clearly traced as the ground of want of success at first, or of death at an early period after the operation. The tables of the situation in which the artificial anus was made, led to remarks on the comparative propriety of Lithe's or Callisen's operations, from which it appeared clearly right to operate externally to the peritonæum on the right side of the body; but the question was left undecided as to the descending colon, so far as the much smaller number of eight cases, compared with twenty, could decide the point; the dangers of peritonitis, the facility of keeping open the artificial anus, and the errors of diagnosis, being discussed *seriatim*, with reference to the choice of the mode of performing the operation. With regard to the latter point of the diagnosis, Mr. Hawkins showed that very few errors appeared to have been committed in deciding whether the opening ought to be made in the right or left colon, and, consequently, that there was no necessity for always operating on the right side, as M. Baudens had advised, even when the obstruction was believed to be in the rectum or sigmoid flexure. But the author brought under notice several cases, showing the difficulty of distinguishing whether an obstruction was situated in the small intestines or in the large, and that even when the peritonæum was opened, the seat of the obstruction had not always been discovered, the difficulty being, as it seemed, liable to be increased still further by the existence or supposed existence of a hernia, of which some instances were also given at the conclusion of the paper.

Mr. Norman begged the attention of the Society to one or two considerations in respect to the diagnosis of such obstructions—the real point of importance and difficulty in the question. These had occurred to him from cases that had come under his notice in practice, and he thought them deserving of attention. In the first place, the existence of diarrhœa prior to the complete obstruction. This had been alluded to as occurring in the case given to the Society by Mr. Adams on the former evening, and was the occasion of doubt and difficulty in a case that he (Mr. Norman) had attended some years ago. The subject, an elderly female, had laboured for some days under an attack of diarrhœa; the purging was frequent, though not profuse; and when it ceased, vomiting, which eventually became profuse and stercoraceous, with tympanitis, and entire cessation of the ordinary fæcal evacuations, showed that there was a complete obstruction of the bowel at some part. This did not seem to be in the great intestine, on account of the large quantity of fluid which could be injected in enemata, and retained, and there was an entire absence of symptoms by which he, or others who saw the patient with him, could determine the situation of the obstruction. The patient died, and on a post-mortem examination, the coils of small intestine were found so matted together by old adhesions, that they could with great difficulty be separated from each other, and at length in the lower third of this gut was found a stricture, occasioned by the contraction of the plastic effusions of an old attack of peritonitis, which had reduced the calibre of the gut to that of a crow-quill; nevertheless small scybalæ, or pellets of solid fæcal matter, were found below the stricture. These had probably passed the stricture in a fluid state, and become more firm by subsequent absorption of the more liquid parts.

Mr. Hodgson said that undoubtedly the diagnosis of these cases was the most important point connected with them, and was surrounded with difficulties. It had been stated that the distended appearance of different parts of the intestines would aid us in pointing out the seat of obstruction, but this was an uncertain sign, and liable to lead us into error, for in fat persons, or where the obstruction had long continued, there would be a general fulness, and no certain information could be gained from this sign. We should be aided much in our diagnosis, however, by a very careful study of the history of the case, going fully into its particulars, and inquiring as to where distension was first perceived and other symptoms. From these inquiries, we might often form a pretty accurate guess where the obstruction was; but the most certain sympton as to whether the obstruction existed in the large or small intestines was to be found in the existence of vomiting and the history of this symptom. When the obstruction was low in the colon, the vomiting did not generally come on until some time afterwards, but when it was situated in the small intestines, it was an early symptom; and if in such cases the vomiting is *not stercoraceous*, the obstruction may be fairly supposed to be in the small intes-

tines. When the obstruction was in the large intestines, the vomiting became fæcal after the obstruction had existed for a shorter time than when it existed in the small intestines. The cases which had been read at the last meeting of the Society bore out this observation. He thought the preferable operation, when it was decided that the seat of obstruction was below that point, was Amussat's, in the left colon; for it was important that the site of the artificial opening should be as low in the intestinal canal as possible. When the opening was made in the small intestines, the patient became imperfectly nourished, and soon sunk. In Mr. Clement's case, in which the opening was made in the caput coli, and the patient lived about three years, and death seemed to have been induced by atrophy, the body, when opened, was found very attenuated, probably from want of that part of alimentation which was consequent upon the process which went on in the colon. It was best, therefore, that the opening, when practicable, should be made in the left colon. When this could not be effected, from the circumstances of the case, then the opening should be made in the right colon. An artificial anus from the small intestines was justifiable, when the patient was informed that its chance of prolonging life to any considerable length was but very small. The objection to Amussat's operation was, that there was a great disposition in the orifice to contract, and to render the passage of the fæces difficult or impossible. This was no doubt the fact in some cases, but the considerations relating to nutrition, above noticed, were sufficient to make an opening in the left preferable to one in the right colon, and still more so, in his opinion, than in the small intestines. After speaking of the services rendered by the authors of the papers to the settlement of the question, he said this operation, as proved by statistics, had been performed with as great a degree of success as many of the great operations in surgery.—*Lancet, March* 6, 1852, *p.* 246.

100.—*Reduction of a Strangulated Inguinal Hernia, the patient being under the influence of Chloroform.* Under the care of EDWARD STANLEY, Esq., F.R.S., at St. Bartholomew's Hospital.—[Chloroform may not only banish the perception of pain, but may also in some cases preclude the use of the knife. Arthur H——, aged twenty-five, a butcher, had been liable to inguinal hernia of the right side for several years. Had worn no truss, as the hernia was small. As he was sitting on his bench, without making any particular effort, the bowel came down and could not be returned.]

At three in the afternoon, shortly after his admission, a consultation was held between Mr. Lawrence, Mr. Paget, and Mr. Stanley. It was thought advisable that no more taxis should be tried, and that an immediate operation would be more conducive to the patient's safety than a prolonged trial at reduction with the assistance of the warm bath, bleeding, tartar-emetic, or tobacco. The patient was therefore taken to the operating theatre, and chloroform administered; the anæsthetic agent took effect without causing any excitement, and in three or four minutes the man was fully under its influence.

Mr. Stanley then proceeded to apply the taxis, and by continuous, gentle, and well directed efforts the tumour distinctly re-entered the abdomen; the spermatic tract remaining somewhat thick from (probably) the sac and a certain amount of moisture. The proper appliances to the groin were then used, and the man taken to bed.

Mr. Stanley took occasion to state to the pupils, that he considered, though no operation with the knife had been performed, that they had now received an important lesson, which went to show how valuable an agent was chloroform in such cases, superseding as it does bleeding, tobacco, &c. He and his colleagues had acted towards the patient exactly as they would have been treated themselves; and it was plain that in similar circumstances, no time ought to be lost with taxis, baths, &c. The patient was discharged, in a few days, in a very satisfactory condition.—*Lancet, Nov.* 29, 1851, *p.* 509.

101.—*New Mode of Reducing Strangulated Hernia.* By DR. THOMAS A. WISE, late Surgeon H.E.I.C. Service.—The following are the particulars I promised to

send you, regarding a new method of reducing strangulated hernia. While I had charge of an hospital in India, an elderly man was brought to it with a strangulated inguinal hernia. After in vain employing the usual means of reduction, I was preparing to liberate the gut with the knife, when a Mussulman gentleman suggested, that the following method should be first tried, as he had seen it successful. As it appeared most simple and effective, I at once proceeded to try it. The patient was placed upon a table, and a long sheet, folded several times on itself, was carried round the lower part of the abdomen of the patient, and twisted on itself in front, and again on the sides, so as to enable an assistant, standing on each side of the patient, to hold the extremities of the sheet, and to pull them gently upwards, or towards the patient's head, while a third assistant held the feet steady, and the surgeon used the taxis.

As the gut immediately above the strangulated portion was superficial and distended with air and liquid, it was drawn upwards with considerable force from the hernial sac, which was assisted by the surgeon using the taxis; when the strangulated portion was immediately reduced.

This simple method may, in a very large proportion of cases, be employed with perfect safety and at an early period, before inflammation and thickening has complicated and increased so much the danger of the operation, which is thus rendered unnecessary.—*Monthly Journal of Med. Science, May,* 1852, *p.* 385.

102.—M. BOURJEAURD'S NEW ELASTIC AND AIR-PAD TRUSS.

It is now some time since our attention was first attracted, at Guy's Hospital, by M. Bourjeaurd's apparatus, which we saw him apply upon a patient of Mr. Bransby Cooper's. The new truss was then highly approved of by Mr. Cooper; but we refrained from recording these facts until time had sanctioned the first favourable impression, and the apparatus had been tried upon other patients. These two conditions have now been fulfilled; a period of several months has passed since the new elastic truss was applied on Mr. Cooper's patient; and we have seen the apparatus used in two other cases at St. Bartholomew's Hospital, respectively under the care of Mr. Lawrence and Mr. Stanley.

From what we have seen, from the high encomiums of the surgeons, and the satisfaction of the patients themselves, we are led to believe that a complete revolution is on the eve of occurring in the systems hitherto followed in the manufacture of trusses; and if M. Bourjeaurd's elastic apparatus and air-pad continue to give as much ease and comfort to patients as has hitherto been the case, it is very likely that they will, ere long, entirely supersede the steel springs and usual pads.

In fact, we were somewhat prepared for this change; for we had seen the principle already carried out in a case of umbilical hernia at the London Hospital under the care of Mr. Curling. The apparatus answered the purpose admirably in that case; and it would in fact have been extraordinary if the use of the elastic band and air-pad had not, by proper modifications, been extended to crural and inguinal hernise. Indeed, with some ingenuity, it is plain that the apparatus may be rendered useful in many surgical appliances, which in their present state must be looked upon as imperfect. Among those that might be named, we would just mention the cushions used along splints in fracture; these cushions being made of caoutchouc, filled with air, and provided with a pipe and stop-cock, may, of course, be made more or less tense without disturbing the parts in the least. We shall now attempt to give some idea of M. Bourjeaurd's new contrivance.

The new truss is only a special application to hernia of an extensive system of effecting compression by means of elastic bandages. The latter, in fact, may be used in numerous medical and surgical cases, where a compressing action is indicated, by being properly modified and adapted. Varicose veins, various kinds of œdema, rupture of tendons, sprains, fractures, &c., are greatly benefited by their use. The texture which is employed is composed of caoutchouc threads, which, according to circumstances, are covered over with silk, wool, or cotton. Bands or strips almost one inch wide are then woven with these threads, and the strips being sewn together in a spiral form, yield bandages which may be applied to all parts of

the body, producing a methodical, regular, and continuous compression, either over the whole surface with which they come in contact, or over any definite region.

Such being the general principle and nature of M. Bourjeaurd's compressing apparatus, the extension of the principle to the purposes of hernial bandages may be easily understood. The new elastic truss is applicable to inguinal, crural, and umbilical herniæ, and consists of a belt about eight inches broad, composed of elastic strips spirally sewn together. To the right or left side of the lower margin of this belt (according as the tumour is situated on the right or left) a thigh-piece made of the same material is adapted, the latter being used to keep the belt infallibly in its place. The belt, however, is itself only the support and protection of the special agent of compression, which is an oval bag of soft and pliable caoutchouc, covered with chamois leather, and filled with air. The latter is blown into it by means of a short pipe and stop-cock, and the pad is fixed on the internal part of the belt, opposite the spot where the compression is required. This spot is mostly the internal inguinal or the crural ring.

The amount of insufflation into this kind of pad will regulate the degree of compression which it is advisable to use; and the continuous action of the pressure is further secured by two inelastic straps, which pass over the pad. These straps may be either loosened or tightened by means of buckles, and this faculty is of very great importance, as thus an immediate and permanent compression may in a moment be obtained. This has been added, in order, as it were, to keep elasticity within limits, and apply pressure on a determined point of a large surface. It will suffice to see the apparatus once upon the patient to understand how it works, and what comfort it procures, especially to those who have suffered by the use of steel springs.—*Lancet, Jan.* 10, 1852, *p.* 43.

103.—ON HÆMORRHOIDS.

By Richard Quain, Esq., F.R.S.

[In the following case of *external hæmorrhoids*, the gentleman had been subject to constipation which baffled all his attempts to overcome, though lately he had taken the confection of senna with some advantage.]

The hæmorrhoids not only descend when the bowels are evacuated, but even when the patient walks a short distance; and when the protrusion takes place, so much distress is felt, that the immediate replacement of the tumour becomes necessary. Hence Mr. — is hindered from entering into society as he would desire; and he is subject to the annoyance of being obliged even to avoid walking with his family.

I found the skin of the anus preternaturally loose; and, with a little effort upon the part of the patient, a complete and thick ring of dark-coloured hæmorrhoids was made to project. The vascular growth starts suddenly out from the surrounding mucous membrane, and its limits are thus clearly defined. The vessels are evidently much thickened.

The hæmorrhoidal mass being brought fairly down, after the action of aperient medicine, three double ligatures were applied, and the part was then replaced within the sphincter. The patient was surprised when told that the operation was ended. He subsequently stated that he had suffered nothing that could be called pain, either during the application of the ligatures or afterwards. There was, he said, a degree of uneasiness, and no more. He slept well that night; was confined to a couch a single day, and went out some way on the next day but one after the operation. The loops of the ligatures were separated on the fourth or fifth day, and the patient left town in a very few days after. He has since then informed me, by letter, that he is quite free from every appearance of the hæmorrhoids and the sufferings they occasioned, but that the old constipation continues to be a source of annoyance.

[The next case is of *internal hæmorrhoids* which had produced frequent bleeding, and much suffering during six years.]

Upon examination, the skin about the anus was found to be prominent, and within this there were seen several dark hæmorrhoids, as well as some of a brighter

colour. The hæmorrhoids were in part visible, without any effort being made to force them into view.

After preparation of the patient for a few days, by regulated diet and the action of aperients, the hæmorrhoids were transfixed with double ligatures, which were secured as in the preceding case. Four such ligatures were applied. After being tied, the hæmorrhoids, though pressed upwards, were still in part visible, owing to their length and size. During the operation no pain was felt except at a single point, where the part to be tied being low down, a very small portion of the skin was included in one of the ligatures. It was only when this ligature was being tied that the patient winced for a moment. Two days afterwards there was a good deal of œdematous swelling of the skin around the anus. The ligatures came away about the fifth day, and in about a week after he left the hospital.

[After this we have another case of internal hæmorrhoids in a lady who had been confined by them for a period of nine years, from pain and frequent bleeding. Dr. Quain says:]

When I first saw Mrs. —, she was pale, emaciated, and languid. She was evidently much depressed by long illness and losses of blood. After the use of an aperient there was considerable prolapsion of the rectum, and several hæmorrhoids were brought into view. There was no evidence of pulmonary or other organic disease. Palliative treatment was evidently here out of the question. The descent of the bowel must be prevented, and the bleeding vessels must be permanently closed.

After some preparation during a few days, double ligatures were passed through some of the hæmorrhoids, previously brought fully within reach after the free action of aperient medicine. In the subsequent progress of the case an abscess of small size formed at one side between the anus and the tuberosity of the ischium; but even with a little retardation of her recovery caused by this accident, the patient left town within three weeks after the operation.

[In the next case bleeding occurred at intervals for sixteen years, in the later years being very profuse, occurring at intervals varying from a period of from four to five days to as many weeks.]

The protruded hæmorrhoidal mass, when forced down for the operation, was large, and upon one side was as thick as a pullet's egg. Florid blood was projected from three round openings, the jets being each as large as a thick pin, and blood escaped very freely. Three double ligatures were passed. After the operation, laudanum was twice given during the evening. The progress of the case was rapid, and in every way favourable.

The hæmorrhoids descend with the mucous membrane, which in this place is but loosely connected with the subjacent or superjacent structure, and not only is the tumour thus formed by the thickened vessels and the blood contained within them, a source of much discomfort, but, if the part be not speedily replaced, the sphincter, thrown by the presence of the swelling into spasmodic contraction, produces a strangulating effect upon the vessels, and hence arise increased turgescence, inflammatory action, and pain.

The object to be accomplished for the cure of such a case is, to bring about adhesion between the mucous membrane and the other coats of the bowel, so as to prevent the prolapsus. The adhesive process required to retain this end may be excited by the removal either of some of the hæmorrhoids or of a portion of the skin and mucous membrane at the margin of the anus. The effect of such an operation is here, as elsewhere, the effusion of lymph, the formation of a cicatrix, and, as a consequence of the process, the agglutination of the parts together. But as regards the two methods of proceeding, I prefer the removal of the hæmorrhoid, because it is not only more direct, but, as I believe, more certain also, the diseased part, the source of the evil, being put out of the way. I have no fear of any unfavourable result, provided the precautions to be hereafter mentioned are observed.

Turning now to the other cases, you will find that, besides the distress produced by prolapsus, there were in these losses of blood likewise. One had been suffering from pain and debility, occasioned by frequent bleeding, during six years continually; another (Mrs. ——) had been, for the same causes, during nearly nine years, and for ten months in each of those years, confined to her room completely prostrated; *while in the* last of the cases the hemorrhage was so profuse as to imperil

life by the mere loss of blood. Upon this subject, considering its importance, I shall offer some further observations before proceeding to describe the operation.

The discharge of blood is a frequent accompaniment of hæmorrhoids; but in the greater number of the examples of this complication that come before us, the quantity is but inconsiderable; and when it is so, especially where the occasional and slight hemorrhage attends upon some general constitutional ailment or organic disease, it is not to be arrested permanently without careful consideration of all the circumstances of the individual case. The escape of the blood usually occurs when the bowels act. At this period, the hæmorrhoids are forced below the sphincter, and the hemorrhage lasts during the protrusion, or, what is commonly the same thing, during the efforts of evacuation. In the last of our cases, the loss of blood had occurred at other times when there was no protrusion, in fact, while the person was walking in the streets. But in this respect, as well as in the amount of blood lost, this was an extreme case. These circumstances are not without their practical indications. Thus, the time for the evacuation of the bowels must be as brief as possible; constipation is to be obviated; and the protruded part is to be speedily replaced. Moreover, it is important that the surgeon should investigate for himself the amount of the hemorrhage. I have found by examination the loss of blood to have occurred at a single time to the extent of half a pint, when the patient was not aware of more than a little oozing. For the arrest of this hemorrhage, how far may local astringents be relied on? To this question, my answer is, that while I cannot aver that they are altogether to be disregarded, I must add that, in the circumstances before us, I have but little reliance on the efficacy of such applications. It is not often that we have occasion to observe the actual escape of the blood from hemorrhoids. Still, I have had in several cases the opportunity of doing this, and in every instance I found that the blood issued from one or more small but distinct openings. Of the bleeding, in the fourth of the cases above cited, it is mentioned that florid blood was projected from three round openings in continuous jets. Bleeding in any other form has not fallen under my observation. I have not seen anything that could be termed an exudation from a general surface. Now, judging by what we witness in other parts, it seems probable that styptics, if directly applied to the bleeding orifice, would have some influence in restraining the hemorrhage in the cases before us; but the difficulty is to make the application directly to the part. In order that it shall with certainty reach the one or two bleeding points in a cavity of no small dimensions, the injection must be used in considerable quantity. If, however, the quantity should be large, it will bring on the *nisus* for evacuation, which ought by all means to be avoided. Again, although some astringents taken into the stomach have, in hemorrhage elsewhere, considerable influence, they have but little if any beneficial effect in the bleeding which attends upon hæmorrhoidal disease. The patient in the last case took gallic acid for some time, but apparently without any good result. Indeed, the influence of the forcing efforts of defæcation upon the venous circulation, the great distension of the vessels when protruded, and the constriction of the sphincter, are likely to render the use of all such remedies nugatory. On the whole, therefore, I rely mainly for the arrest of the hemorrhage, on the replacement of the vascular growth within the sphincter, and the maintenance of the horizontal position of the body; but if the loss of blood be considerable or continued, the operation for affording permanent relief must be performed. The process by which it has been already stated the prolapsion is prevented, is sufficient for the cure of the hemorrhagic tendency likewise. The plan of operating is therefore the same, only that the direct action upon the hæmorrhoids is even more necessary here than when the prolapsus is not attended with bleeding.

Now, as regards the *operation*:—In order that the healing process should go on prosperously afterwards, it is necessary, in this as in all other cases in which any delay is admissible, that the patient's health should be well looked to before the operation is proceeded with. It was on this principle that one of the patients who was faint and exsanguined when brought to the hospital, was allowed to continue for a time under care before the operation was undertaken; while, inasmuch as there was nothing in the state of the health of the other cases to forbid the operation, the free action of an aperient, and the regulation of diet for a couple of days, was all the preparation that was thought necessary.

The ligature used is of thick silk, and it is carried double through the middle of the hæmorrhoid, or of the portion of this to be acted upon, by means of a needle furnished with a handle, or even with a common bent needle. Each part of the ligature is to be tied at the side, and as firmly as possible, so as to destroy the vitality of the included hæmorrhoid. The extent of the mass to be included I am in the habit of regulating with a tenaculum, committed to the hand of an assistant after its point has been inserted. The same purpose may be attained by means of a hare-lip pin, passed transversely, and made to penetrate at both sides where the ligatures are to lie. When the purpose has been served, the pin, or tenaculum, is of course withdrawn. Caution is necessary, that none of the skin be included in the ligature; and should any happen to be within the space to be acted on, it must be divided in the track of the ligature before it is tied. It is thus that pain during the operation, and suffering afterwards, are to be avoided. The gentleman whose case is first cited, stated that he felt no pain during the operation or after,—only a degree of uneasiness; and he was able to employ himself in reading immediately after. The second patient, however, felt some pain, because a small point of skin happened to be comprised within one of the ligatures.

For the proper performance of the operation, it is best that the bowel should be prolapsed, and this will be effected after the use of an aperient or an enema, or even in some instances without these, by the efforts of the patient. The co-operation of the patient is required to keep the part properly within reach. This circumstance has its importance when the propriety of administering chloroform is to be decided upon, for when insensibility is induced, and the voluntary efforts of the patient are thereby necessarily wanting, the hæmorrhoids, though previously brought into view, are commonly at once withdrawn within the sphincter. It then becomes necessary that the bowel should be dilated and the hæmorrhoid drawn down with a pair of "fenestrated" forceps. This plan, however, is not satisfactory. It does not allow of the precision in applying the ligature that is desirable. On this account, when obliged by the wishes of the patient to administer the anæsthetic agent, I have, in some instances, before its administration was begun, passed a couple of ligatures through the hæmorrhoids, (after prolapsion had been brought about,) leaving them of course quite loose till after insensibility was induced. But in cases of well defined internal piles I much prefer that the patient should not be rendered insensible, for, besides the inconvenience caused by the want of his co-operation during the operative proceeding, it is to be taken into account that the pain, as already mentioned, is really inconsiderable, while the chloroform in this, as in some other cases, frequently occasions no trifling amount of after-discomfort.

After this operation, as after most others, it is well to administer an opiate if the patient should feel uneasiness. The action of the bowels is not to be speedily looked for, but, should they not act spontaneously in three or four days, I direct an enema or a mild aperient to be administered. There is occasionally a difficulty in evacuating the urine. Hot fomentations and a dose of opium will expedite the removal of this distressing incommodity. Very rarely indeed is it necessary to introduce a catheter.

The sequel of the cases cited at the commencement of this lecture may be considered the type of the result of the operation in others. An unfavourable, or even unsatisfactory termination, is of extremely rare occurrence. Sir B. Brodie stated, in 1835, that he had then seen but two examples. Mr. Copeland (1824) mentions one, and it has fallen to my lot to witness a single instance also. In judging of such results, it must be remembered, that there is scarcely an operation, be it ever so trivial, that may not, in extraordinary circumstances, prove fatal, just as it needs no long acquaintance, with the practice of the hospital, to have occasion to observe serious consequences and even death, to follow from a slight accidental wound or or abrasion; nevertheless, one has little apprehension about such an operation or injury. It is right, however, to use experience—our own, and that of others, so as to reduce the possibility of such an event to the lowest degree possible. Now, in the case which I had occasion to examine *post-mortem*, there was serous effusion into the cellular membrane of the pelvis around the rectum, and the lower part of the bowel was in a sloughy state. There had been, before the operation, repeated losses of blood to a considerable amount, and the person was in a feeble state, though able to go abroad and attend to his occupations. Several double ligatures *(five, I believe) had* been applied to the hæmorrhoids, which were entirely of the

internal kind. A similar condition (of diffused cellular inflammation)is mentioned by Sir B. Brodie, as present in a body which he examined under the same circumstances. With my present experience, my conclusion is, that it is important, in all cases, to avoid applying ligatures in large numbers, and that this rule is especially important where the patient is in impaired health, or debilitated by loss of blood. I am disposed to limit myself to the application of three double ligatures, though I have, in several cases, used a greater number with the best result. The smaller number will, in most cases, be sufficient to prevent the prolapsion afterwards, and the hemorrhage as well; but even should it not have this effect altogether, it is better to have to return to the application of a ligature or two more, than to do too much even once in life. I have acted in several cases on this principle, where the state of the patient's health seemed to require such caution.

External hæmorrhoids vary much in size, from a swelling little more than discernible to a tumour the size of an egg. When of any magnitude they are a source of much inconvenience on account of interfering with the sitting posture, and when in any degree inflamed, they are inconvenient for the same reason; and, moreover, they are then productive of much pain, especially during the evacuation of the bowel. In the inflamed state the skin is at the same time affected, and every disease accompanied with excited action in this part causes much suffering, owing no doubt to its organisation. The distress, too, is augmented by the spasmodic action of the sphincter, which we shall find usually accompanying irritation or inflammation in its neighbourhood. It is to the internal variety of hæmorrhoids, rather than the external, that the tendency to bleeding belongs. The comparative exemption of the latter from hemorrhage is probably owing to the thicker and more solid covering of the skin; while the mucous investment of the internal piles is in some cases exceedingly attenuated,—so much so that the vascular growths seem bare, at the same time that these are subjected when prolapsed to the strangulating effect of the sphincter: another reason for the hemorrhage.

The acute inflammation of external hæmorrhoids is treated as would be the same condition in other parts. As the suffering is so much greater than the same amount of inflammation would excite elsewhere, (except probably when the eye and ear are concerned,) you ought to be especially careful in superintending the arrangement of the proper appliances. All the applications,—the poultice, the spongio-piline, and so forth, with their proper application and support—small matters as these may be regarded—are as much the care of the surgeon as the steps of a "great" surgical operation. So indeed is everything which tends to abridge the suffering of the patient. As the inflammation ends, the veins shrink, and there is, after a first attack, little other remains of the previous swelling than the fold of skin of, it may be, larger size than natural; but by repeated attacks the twisted vessels are thickened and at the same time united into a lump by new deposit, and thus a permanent enlargement is formed.

When a clot of blood is contained in a large dilated vein, the termination will be the same as that described in the last statement. The blood is gradually altered, becomes adherent to the vessel; this is obliterated, and the whole shrinks into a small compass of little more than cellular structure. The cure is expedited in this case by laying the lump open. I do not, however, advise that the person should, even after this trifling operation, be allowed to walk about. Hemorrhage to some extent may occur; not indeed so as to be a source of uneasiness, but still enough to give the appearance of bleeding in the dress of the patient, or to cause inconvenience, even faintness, in a delicate person.

The little abscess which forms in connexion with external hæmorrhoids, will sometimes continue a long time unhealed, if left to itself. It is to be freely laid open, or the little swelling, when it is the only disease, may be removed altogether with a pair of scissors. It must be remembered, that any meddling with even such small swellings as have been here adverted to, is attended with much pain; and any thing that is to be done, must be done with full preparation against failure from the restlessness of the patient. The use of chloroform may be required, even for these small operations, in some nervous persons.

The removal of the hæmorrhoids is rendered necessary by the inconvenience of the tumour, or by the frequent occurrence of increased swelling. The plan of the operation is as follows:—While the patient lies on one side upon the edge of a bed

or couch, and an assistant separates the nates, the swelling, with its cutaneous covering, is raised with a pair of suitable forceps, or a vulsellum, and the whole is removed in a longitudinal fold, with a pair of firm scissors, curved on the side and blunt at the end. Caution is necessary as to the extent of skin that is removed, for if this should be too freely taken away, the orifice of the bowel may be narrowed so as to produce inconvenience afterwards. For this reason, I have, in a case where the tumours were unusually large, turned back the skin before proceeding to the extirpation; but to this point reference will again be made when prolapsus is under our consideration. It is not allowable, in any case, to include in a ligature the external hæmorrhoid, together with the skin that covers it. Such an operation would give rise to extreme suffering, and might even compromise the life of the patient. The hemorrhage which usually accompanies the operation may be arrested in the usual manner, viz., by tying the little arteries that bleed; but bleeding is liable to come on in an hour or two, or even later, and the application of a ligature at this time is productive of great pain. The influence of the chloroform has then passed off; moreover, it is in every way unpleasant, especially in private practice, or in the case of female patients, to have to return to an operation. It is on these accounts that I have sought to avoid the need of any further process, by passing at once a pin through the bleeding part, and over the ends of this a few turns of ligature with sufficient tightness to control hemorrhage—using, in fact, "a point of interrupted suture." A scrap of lint is inserted in the wound, if it be desired to prevent immediate union. The pin is withdrawn in twenty-four or forty-eight hours. When first led to have recourse to this expedient by the necessities of a particular case, in which I performed the operation with little assistance,—a nervous lady being the patient,—I used the hair-lip pin in common use, but since then I have had one constructed for the purpose. This is blunt at both ends, has a moveable point, like the hair-lip pin formerly used, and a hole or eye at one end, into which a thread is inserted to assist the removal of the pin. The object is, that this should not prick the integument in the neighbourhood—gathered in, as all is here, with the sphincter—and that it may be withdrawn, even though hidden when the parts are swollen with the œdema which follows the operation. To facilitate its removal, the pin is to be inserted as much in a longitudinal direction as possible.

In the works of some writers of authority will be found strong objections to use the ligature in the treatment of hæmorrhoids, and in other works objections equally strong are raised against excision. The observations I have made will lead you to the conclusion, that my own objection to each of these means is of the strongest kind, unless when used in the form of the disease to which it is adapted,—excision being inapplicable to the internal hæmorrhoid, and the ligature to the external. But if means should be taken to prevent the evils of these operations respectively, namely hæmorrhage in the former, and inclusion of the skin in the latter, then the obection falls to the ground. Accordingly the internal hæmorrhoid may with safety be excised—snipped away with a pair of scissors, if the pin I have before described be inserted, and a thread drawn over it, so as to guard against bleeding. With the string attached to it, the pin is to be drawn away in two or three days. The same end may be attained by various other means. So likewise the external hæmorrhoid may be tied without ill consequence, provided the skin be completely divided in the track of the ligature before this is drawn tight.

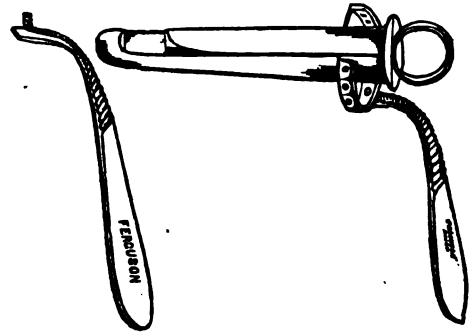
Other means than those recommended in these lectures have been used for the removal of hæmorrhoids, viz., the actual cautery and a mineral acid—the nitric acid. The latter, I believe to be a useful application. I have often found it so in the treatment of some forms of nævus, but as I do not like changing from what I have long found an effective and safe plan of treatment, I have not hitherto used the acid, or any similar application for the cure of hæmorrhoids. As regards the actual cautery, I am desirous to mention, that a very ingenious method of applying it has lately been introduced by Mr. Marshall, and whenever it is desired to remove hæmorrhoids by this process—*i. e.*, by searing them off—I am satisfied that Mr. Marshall's plan is the most convenient form of effecting the object. According to this plan, the wires which are used are heated by means of a galvanic battery.—*Med. Times and Gazette, Jan.* 24, *p.* 79, *and Feb.* 14, 1852, *p.* 154.

104.—INFLAMMATION AND ULCERATION OF THE RECTUM: IMPROVED SPECULUM RECTI.

By Wm. Coulson, Esq., Senior Surgeon to St. Mary's Hospital, &c.

[At a meeting of the Medical Society of London,]

Mr. Coulson said, that in September last, he saw, with Mr. Haden, of Sloane-street, a patient, aged thirty-four, who passed a semi-solid fæculent motion once in twenty-four hours, tinged with mucus and blood, and in addition to this, within the same time, four or five evacuations consisting solely of small quantities of mucus and blood. On examining the bowel with the speculum, it was found that the mucous membrane was destroyed to the extent of two inches from the anus, and pus and blood were seen exuding from the ulcerated surface. Various local and constitutional remedies were employed with little relief except a slight diminution of the discharge. It was then suggested that the decoction of tormentilla should be tried; three ounces of this root in a pint and a half of boiling water were boiled down to a pint, and four ounces of the decoction were thrown up the rectum twice a day, and retained each time a quarter of an hour. Under the use of this remedy the pus and mucus gradually diminished, and within five weeks the ulceration had completely healed; the only medicine taken during this time was a little castor oil, to keep the bowels loose. Mr. Coulson believed the rectum to be occasionally the seat of inflammation, attended with muco-purulent discharge, which, if unchecked, proceeds to the destruction of the mucous membrane of the bowel, and to the formation of abscesses in the neighbourhood of the anus. A frequent desire to go to stool exists in these cases; and unless this be yielded to at once, the motions come away of their own accord, loose, and mixed with blood and mucus; at last the patient's health gives way, and he is worn out by continued suffering. Mr. Coulson showed a preparation taken from a patient who had died of this complaint: the cellular tissue round the anus was hardened, the mucous membrane of the rectum completely destroyed, and the internal surface of this bowel presented elevated hypertrophied muscular fibres, between which there were several openings communicating with external abscesses, so that these in fact were the result of the disease of the interior of the gut—the diseased action without being continuous with that within. The ulceration of which he had been speaking, was not to be confounded with the fissured rectum which often occurs from mechanical causes, or the ulcerated rectum which is sometimes found in persons labouring under a syphilitic taint. These conditions of the bowel were very painful, and the source of great distress to the sufferer, but they easily yielded to remedies,—as the black oxide of mercury ointment (one drachm of the black oxide to an ounce of lard) or if this failed, to a division of the surface of the ulcers,—and when left to themselves did not destroy the patient. Mr. Coulson said it was most desirable, in all diseases of the rectum, to make an examination of the bowel with the speculum; and he would take this opportunity of showing one which was a modification of Mr. Hilton's and Mr. Curling's. The difference consisted in the handle being loose, so that the same instrument could be used for any part of the bowel, which was not the case when the handle is fixed. This alteration was suggested by Mr. Hovell, of Clapton.



The instrument is of a conical form, four inches long, three-quarters of an inch at the small extremity, and an inch and one-eighth at the large. Upon the larger end is a ring, two inches and a half in diameter, with one-third of its circumference removed. In this ring are seven holes, half an inch apart, into either of which the handle can be screwed, thus permitting the handle to be placed at any inclination to the opening in the speculum. This is of use sometimes, for when the handle and the opening are in the same line, the latter is in the way of the patient's thighs. In the speculum is an opening, one inch at the largest end, and three-quarters of an inch at the point. Into this is fitted a metal plug, which just fills up the opening, so that a perfectly conical tube is formed for introduction. After introduction, the instrument is turned round until the plug is just opposite the ulcer; the latter is then withdrawn, and the diseased portion is then very clearly seen falling just into the opening of the speculum. No attempt to introduce the plug while the speculum is in the rectum should be made; but the finger should be passed into the speculum for the withdrawal.—*Lancet, Nov.* 29, 1851, *p.* 515.

ORGANS OF URINE AND GENERATION.

105.—ON THE ANATOMY AND THE DISEASES OF THE PROSTATE GLAND.

By John Adams, Esq., Surgeon to the London Hospital, &c.

[Mr. Adams states his implicit belief that the urethra is completely surrounded on all sides by the prostate. The intimate structure of the prostate he says is not complicated, and describes it as follows:]

"Briefly, it may be said to be composed of minute terminal follicles, opening into canals or tubes, which unite together to form ducts, which open in an oblique manner on the prostatic portion of the urethra. The orifices of the prostatic ducts are situated principally close to and around the most elevated portion of the veru montanum, in the form of a crescent, the larger ducts on the side, and the smaller on the posterior aspect of this body. If a longitudinal, vertical section is made, many of the ducts of the prostate are seen passing upwards, towards the under part of the veru montanum, in a straight direction: the interior of some of them being slit open in the section, whilst others pass obliquely beneath the mucous membrane for some distance prior to their termination. They vary in number from ten to fourteen, but as many as thirty have been seen. Their diameter ranges from one-sixth to one-fourth of a line. It sometimes happens that two or more ducts unite, and open by one common orifice, large enough to admit the end of a small probe.

"I think the fact of the prostatic secretion being naturally, as I believe, acid, is a circumstance of some interest. The secretion of the testes is well known to be alkaline. Is it not probable that the reaction of the prostatic on the seminal fluid may be of use in the maintenance of the fluidity of the latter? The idea is somewhat confirmed by the fact, that in women the acid secretion of the vagina prevents the coagulation of the menstrual blood, and thus favours its discharge. This has been proved by Mr. Whitehead, who found, that if the menstrual fluid was received directly from the os uteri into a speculum, it coagulated like ordinary blood."

[In speaking of the diseases of the prostate, inflammation holds a chief place. The Reviewer differs from Mr. Adams in supposing that the inflammation of the prostate in gonorrhœa is often the result of attempts to cure the disease suddenly. There can be little doubt that, as Ricord has stated, in the first instance gonorrhœa is confined to the first two or two and a half inches of the anterior portion of the urethra, and is capable of being safely cured by the nitrate of silver injection, as also recommended by Ricord. Mr. Adams goes on to say:]

"I have little doubt that the discharge of what is very commonly termed a gleet frequently depends on an increased and altered secretion of the follicles of the prostate, and hence the great difficulty so often experienced in the cure of gleet by the use of astringent injections; for it is difficult, nay, even often impossible, to *reach* by these means the true seat of the disease; whereas the exhibition of the *chia turpentine in five-grain* doses often puts a stop to the discharge instantly;

acting, as I presume it does, specifically on the prostatic portion of the urethra, and on the ducts and follicles of the prostate itself."

[The remarks of Mr. Adams upon chronic prostatitis from masturbation are graphic and sensible, and perhaps the most correct account which has ever appeared in print. He relates the following case:]

"Whilst writing the above, a gentleman, apparently in perfect health, called on me in a state of great distress of mind, having been induced to consult an advertising quack for supposed spermatorrhœa. I found that the only disorder he laboured under was an occasional nocturnal emission, which took place about once in a fortnight or three weeks; this, together with the escape of half a teaspoonful of glairy fluid during the evacuation of the rectum, when the bowels were costive, although his general health was not affected, rendered him morbidly sensitive to his supposed infirmity, and induced him to seek advice. The empiric made a great parade of examining his urine, and having received some of it into a glass, he introduced an hydrometer, and showed him what he said was the seminal fluid collecting around the bulb. When I examined this gentleman with the catheter, I found that the prostatic portion of the urethra was rather more sensitive than natural, and that the orifice of the urethra at the glans was a little reddened, and the lips of the urethra agglutinated by mucus. I examined the rectum with my finger, but could detect nothing abnormal about the prostate or neck of the bladder. I directed him to use a cold water injection night and morning, to take ext. conii, grs. v., omni nocte, and a little liq. potassæ in infusion of gentian. His urine was acid, but not especially so."

In reference to these cases the author also observes:

"I cannot believe that any escape of seminal fluid ever happens without the characteristic signs of ejaculation, however modified; and in these cases such signs are frequently altogether wanting."

The so-called cases of spermatorrhœa are very common. The real disease, which might be called paralysis of the genital organs, is, in our experience, equally rare. The first-named is readily enough cured by the application of the argentum-nitratum, as recommended by Lallemand; the cause of the last lies deeper, and is little influenced by topical remedies.

The experience of a large hospital has brought us numbers of these cases; and we deliberately repeat our conviction, that spermatorrhœa does not really exist in a tithe of the examples in which it is predicated. The constant escape of the seminal fluid with the urine, as often described, is, we believe, a fallacy; and as for the evidence which is sometimes adduced in support of such an opinion, that of a greasy pellicle floating on the top of the urine, everybody knows that this appearance is commonly occasioned by the presence of minute crystals of the phosphate, and that patients who think themselves impotent, or about to become impotent, are persons extremely liable to mental depression, and phosphatic urine with its rainbow colour and rapid decomposition.—Mr. Adams thus accounts for the serious effects which the habit of masturbation induces in the system:

"The first effect resulting from the frequent seminal emissions thus voluntarily induced by the patient without the stimulus of natural copulation, is an irritable state of all those parts sympathetically called into action, as the extremities of the *vasa ejaculatoria* and adjacent portion of the urethra, and the prostate gland; the urethral membrane at this part is endowed with exquisite sensibility; I have no hesitation in affirming it to be the most sensitive part of the canal. This sensibility obviously depends on the large supply of nervous filament distributed about the neck of the bladder; for although it is not possible to trace with the scalpel the ultimate termination of such nervous filaments, their destination must be the point referred to. Hence arises the pain, or at any rate the unpleasant sensation, experienced on the introduction of the catheter even in the healthy condition, and the acute and stinging sensation when inflammation exists in the canal. Through this nervous distribution arises the sympathy of this part with the bladder, vesiculæ seminales, and testicles, on the one part, and with the glans penis on the other; a sympathy illustrated by the desire the patient experiences to evacuate the bladder the moment the bougie or catheter reaches this sensitive part; a sympathy illustrated also by the seminal discharges arising from the irritation of this part by the introduction of foreign bodies, when the natural irritability of the

sentient papillæ of the glans penis has been exhausted by the long-continued practice of masturbation, of which the unfortunate case mentioned by Richerand affords so graphic an example. This nervous supply, derived both from the cerebro-spinal and ganglionic systems, affords a ready clue to the explanation of those general phenomena attending these cases; thus, to speak of natural sympathy, the fainting and sickness, so often induced by the first introduction of the bougie, result from the irritation propagated to the heart and stomach by the ganglionic nerves; hence, also, to speak of unnatural sympathies, depend the pain in the loins in onanism, and the deep pain in the dorsal region. The sympathy between the testicles and kidneys is still further anatomically explained by the nerves to both being derived from the same source; the irritation of the kidneys, as exemplified by the copious secretion of limpid urine, results from the same cause; so further, the general nervous exhaustion, as shown by the weakness in the gait, by the *tinnitus aurium*, by the *muscæ volitantes*, and by the general indisposition to mental exertion under the same condition; it is the derivation of the nerves of the testis from the renal plexus, and the connexion of the latter with the cerebro-spinal axis in the lumbar region, that explain the draught, so to speak, upon the central nervous system, which is constantly taking place under the repeated and unnatural excitement of the organs of generation thus constantly indulged in."

His treatment of these cases, besides the local application of caustic, consists in the exhibition of conium and soda in the infusion of gentian, cold bathing, fresh air, and almost entire abstinence from alcoholic fluids. In reference to wine he very sensibly observes:

"I believe the internal use of alcoholic drinks to be most injurious, and that it is infinitely better to deter the patient from the use of wine, or even porter, altogether, than to give them to the extent of stimulating the heart's action."

In our own practice we have found much benefit, after quieting the patient's mind and getting his secretions into good order, from a mixture containing small doses of the tincture of cantharides and the sesquichloride of iron in a bitter infusion. With this it is often necessary to give an opiate at night, and to prohibit the patient entirely from taking any sort of *beer*, which we regard as far more injurious than wine or spirit. On such a plan as this it is often unnecessary to resort to the nitrate of silver, which, we suspect, sometimes leaves behind it unpleasant results.

Hypertrophy is the great change to which the prostate gland is obnoxious, and is so common after the age of fifty, that Mr. Adams says it may be almost regarded as one of the necessary contingencies of old age, supervening in a manner wholly independent of inflammation. The condition of the bladder varies remarkably in this disease; occasionally, it is so contracted and thickened as to hold but a very small quantity of water, and in other cases is so much expanded as to hold many pints.

In the first of these conditions, Mr. Adams believes that there has been inflammation of the prostate, extending to the bladder, and *sudden* increase in the size of the gland; and in the second, that the increase has been gradual, and accompanied by compensative dilatation of the bladder. This dilatation, proceeding in every direction, presses sometimes on the rectum, and produces obstinate constipation, giving rise to that pouch which is so convenient a receptacle for small putrefying quantities of urine, or for calculi. There often happens, also, a hernia of the mucous coat of the bladder through its thickened muscular fibres, which materially aggravates the patient's danger; for these cysts or supplementary bladders answer admirably to contain the urine, but, being destitute of muscular fibres, are unable to expel their contents, which, rapidly decomposing, inflame the bladder, and not unfrequently set up the peritoneal inflammation, which finally kills the unfortunate patient. The author lays particular stress upon what is called the *fluttering blow of the bladder*, as a pathognomonic sign of this condition; and as this is an important practical point, we shall present our readers with his exact words:

"As this is a subject in a great measure passed over by writers on diseases of the urinary organs, I shall take the liberty of dwelling somewhat upon it. So far as I have observed, these cases are usually attended with pain about the region of the pubes, and in the perineum, and along the urethra, especially after the evacuation of the bladder; there is generally more or less irritability of this viscus evinced by a frequent desire to pass water; hence the surgeon's attention is directed *to the state of the* bladder; the catheter is introduced—it may be without difficulty,

or at any rate with no more difficulty than commonly attends its introduction when the prostate gland is hypertrophied; and on the escape of some urine, the flow of water suddenly stops, and *a fluttering blow* is struck against the point of the instrument, as if a solid body came in contact with it: the surgeon, believing that he has drawn off the whole of the urine, is about to withdraw the catheter, when a small quantity more escapes, and perhaps the blow is repeated. The impression first conveyed to the mind of one unaccustomed to it is, that there is a stone in the bladder; but the experienced hand at once detects the nature of the case, or at any rate the idea of the existence of stone is at once removed from his mind. Mr. Guthrie suggests that these are the cases which have been so often mistaken for stone in the bladder, and in which the operation for lithotomy has been attended with a cure of the symptoms from the division of the neck of the bladder. Although such may have been the case occasionally, I am under the impression, that in those cases in which lithotomy has been unnecessarily performed, there is an hypertrophied condition of the *detrusor urinæ*, and that the deceptive sensation which favours the impression that calculus exists, arises from the grating of the point of the sound against the hard *rugæ* of the bladder, formed by the columns of this muscle increased in size and density. It is not long since that I was in attendance on a valued medical friend, who was labouring under all the symptoms of hypertrophied prostate, with its concomitant horrors, the disease approaching rapidly a fatal termination, when his medical attendant in the country assured me that he had detected the cause of his disease in the existence of a stone in the bladder. On introducing the catheter, I was at once convinced that the opinion was erroneous: I felt the fluttering blow upon the catheter, and ventured to diagnosticate a considerable pouch in the bladder: my opinion was verified on the examination of the body after death, which took place soon after."—*Brit. and For. Med.-Chir. Review, Jan.* 1852, *p.* 82.

106.—ON STONE IN THE BLADDER AND ON LITHOTOMY.

By William Fergusson, Esq., F.R.S., &c.

[In a clinical lecture, after detailing the heads of four cases of stone upon which he had lately operated, Mr. Fergusson makes the following observations:]

Usually, in the cases operated on by me, the water comes away by the urethra on the second day, and continues doing so for two days,—then passes all by the wound until about the ninth or tenth day, when it again comes permanently by the urethra; the reasons for these changes are, that a certain amount of inflammation and swelling of the deep part of the wound takes place immediately after the operation, which is sufficient to close the opening for a short time: then when these subside, the urine again flows through it. I think I may say, that in my own cases the urine finds its way again through the natural passage at a somewhat earlier period than is the case with some other operators, and this I attribute to the smallness of the opening I make in the neck of the bladder.

I must now refer to the particular kind of operation which has been performed in these cases; it is the operation of Cheselden,—and is termed the lateral operation of lithotomy, in contradistinction to other operations for stone, such as the high operation—mesial, recto-vesical, bilateral, all of which have been resorted to for the cure of this painful disease. The history of the operation of lithotomy is exceedingly interesting to those who are anxious about the literature of this part of surgery.

Cheselden had various modes; but it is evident that what he practised most frequently and most successfully was the lateral operation, wherein he divided the prostate gland and neck of the bladder as he pushed the knife towards the viscus. Such a proceeding is often now termed Liston's operation. Where this last named great and lamented surgeon learnt his method of performing it, I cannot tell you; certain it is, that when he first began to operate in Edinburgh, the proceeding differed from that which was generally performed in his time; and he was so dexterous, and showed his dexterity before so many pupils and surgeons, that numbers have been induced to follow in his steps, and, probably out of respect for their teacher,

have called it by his name. Perhaps the chief feature of difference between the operations of Cheselden and Liston consisted in the shape of the knife, the situation of the groove in the staff, and in the shape of the forceps; in all these points Mr. Liston differed from Cheselden. I think I ought to mention another feature of difference; this was, that after the operation was performed, Mr. Liston introduced a tube through the wound into the bladder, and kept it there, so that the urine should flow through it. This practice, although an old one, was revived by Mr. Liston. In the work of Scultetus, you will find a representation of this tube.

Notwithstanding all that has been written and practised on lithotomy, frequent attempts have been made to improve the operation; these attempts have been directed to the cutting instruments, as to their length, breadth, and shape; much attention also has been devoted to the shape of the staff. Here you will perceive, that in these two staffs I hold in my hand, the groove is differently placed; in one it is on the convexity of the curve, in the other it is on the left side. Surgeons differ even on this matter, although you see it is but a slight one.

The operation which I have performed is such as I was in the habit of seeing Mr. Liston practise, with only one difference, which I will mention presently. I will now describe the operation as you have seen me do it. In the first place, I always make a point of introducing the staff myself when I am going to operate; this should not be left to another person, for if any mishap occurs, the fault is sure to be thrown upon the unlucky man who holds the instrument. The operator himself ought always to be responsible for every step of the operation. When the staff is introduced the patient should be tied up, but not before. The surgeon now takes a glance at the perinæum, introduces the finger into the rectum to ascertain the position and size of the prostate gland and the condition of the gut itself, which ought to be unloaded previously by an enema. Then, having all his instruments by his side, the principal part of the operation is proceeded with. The knife I use is a scalpel, with a cutting edge only half-way down,—such, indeed, as Mr. Liston used, and which goes under his name; and, curiously enough, I have the opportunity of showing you the very knives which he was constantly in the habit of using some twenty years ago. The incision is now commenced, about one inch and a-half in front of the anus on the left side, and carried downwards towards the ischium to the extent of three inches, or even four in a fat subject. This incision divides skin, fat, and superficial fascia. Then it is carried on between the erector penis and the accelerator urinæ; and, in the triangular space bounded by these muscles and transversus perinei, the operator, at the second or third incision, can place his finger on the staff, and feel the groove through the membranous portion of the urethra. He now puts the point of the knife into the groove, and pushes it along until it has passed through the prostate, keeping its cutting edge outwards and a little downwards towards the ischium; then, generally, there is a gush of urine, by which he learns that the blade has got into the bladder. The finger is then carried into the wound,—the tissues gradually yield before it,—and, if the operator has a nice touch,—which is so necessary here,—he will be able to distinguish the mucous membrane of the bladder from the rest of the wound. Having now got the finger into the neck of the bladder, he cautiously dilates the opening, by moving his finger round, and at the same time feels for the stone; these objects being attained, the staff is taken out and the forceps being slowly introduced as the finger is withdrawn, the stone is seized and extracted. Now, the peculiarity in my own operation is this, that I make the smallest possible opening in the neck of the bladder, so as just to admit the forceps into it. Many lithotomists make a very free opening; and some use this instrument, which is termed a blunt gorget, and pass the forceps along its groove; but I prefer using my finger as a guide, withdrawing it gradually as the forceps make progress; then, just as the blades enter the bladder, a gush of urine takes place, at which instant I open them, and by a slight twist of the wrist, usually catch the stone at once, when, with the utmost care, I draw it gradually downwards until the tissues yield, and it is removed. As I told you before, some advise free incisions; but I only notch the prostate. I consider it a much safer plan to make a very limited cut in this part. I believe the data we have of the operation go to show, that the most successful lithotomists were those who made small incisions into the neck of the bladder. Martineau made a small *incision, dilating it afterwards*; so did Liston. If necessary the opening can be

enlarged; but you may consider it a much safer plan to make a small wound with the knife, and then to stretch it well with the finger: it is extraordinary how the notch will enlarge by dilatation.

Perhaps the chief difference between Mr. Liston's practice and my own consists in the treatment immediately after the operation, and this is in reference to the employment of the tube. I used it in all my early operations, but I had frequent occasions to think that it caused both physical and mental discomfort. The patient had an idea, in wearing the tube, that the whole of the operation was not finished; consequently I, although an Edinburgh pupil, ventured to differ from the surgeons there upon this point. I left the wound entirely to itself, as was the custom with Cheselden, Martineau, Key, Green, and other eminent lithotomists; and I was so well satisfied with this plan, that I now never use this instrument. My friend and former house surgeon at Edinburgh, Dr. Richard Mackenzie, has recently given up the use of the tube, and he writes me, that he is well pleased with this practice.

The rest of the treatment necessary, after the operation, must be carried on according to those general principles of surgery which guide the surgeon in other matters; and any one who is thus well-informed will be able to understand, and be prepared for, those mishaps which will every now and then occur after such a proceeding.—*Med. Times, Dec.* 20, 1851, *p.* 635.

107.—ON SOME DIFFICULTIES OCCURRING IN A CASE OF LITHOTOMY.

By Richard Quain, Esq., F.R.S.

[The mode of operation in lithotomy is sufficiently well established—that the cases of unusual difficulty are those which only require notice. The patient in the following case was admitted into University College Hospital on the 10th of Dec. He is worn and weak, but the symptoms of stone are well marked. His urine is acid. Sp. gr. 1016; contains albumen, but not in large quantity.]

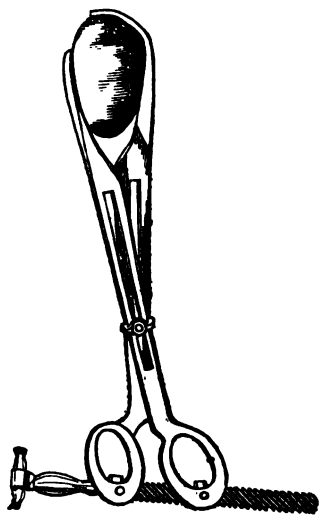
Jan. 8. The patient being brought under the influence of chloroform, and placed in the usual position, the integuments were freely divided; and then, after two short incisions, the knife was lodged in the groove of the staff, and passed onwards to the bladder. The staff, a large curved one, such as Sir C. Bell, in the early part of his career, and Mr. Liston continually used, was held at first so as to be somewhat prominent towards the perinæum, and when the knife reached it, the position of the staff was altered so as to bring up its ends from the perinæum and rectum. So far every thing was in the usual course, and easy. The prostate was found to be enlarged, and it was not till after the forceps had been passed and its blades opened, that a gush of urine took place. The stone was at one side, behind the enlarged prostate, but it was with some management brought towards the middle and seized. When the forceps was drawn forwards, the prostate was pressed, as it were, into the way of the stone. Hereupon the gland was divided with a probe-pointed bistoury on the right side, and more fully on the left side too. But this expedient was ineffectual for the purpose. The forceps, it was found, could not be made to reach so far upon the stone as to give much power by its hold, and a good deal of power was requisite to draw the large stone through an enlarged and indurated prostate. The instrument slipped repeatedly; but that there was no want of force in grasping the handles with a view to prevent the slipping of the blades may be inferred from the fact, that the ends of these were straightened against the stone. Larger forceps were tried, but ineffectually; and the failure is, I believe, to be accounted for by the stone being too long to be taken fully within the grasp of the instrument, and too flat, as well as too hard, to be held with the blades merely resting against it, *i. e.*, by these circumstances added to the resistance of the prostate. In short, after several trials and much exertion to keep the forceps from slipping from the surface of the calculus, or to break it by pressure, I was unable to effect the last step of the operation. I may observe here, that while the stone was being brought forward, a small piece of one of those growths from the back part of the prostate, so common in aged persons, was loosened, and I removed it with my finger. The operation might have been completed speedily with a larger

division of the deep structures; but this course I consider wholly inadmissible, for urine would thereby be allowed to infiltrate the cellular membrane of the pelvis, and the death of the patient would have been the almost inevitable result. The only resource was, therefore, to break the stone into fragments, or to draw it forward by dilatation of the gland and neck of the bladder, for I put it out of the question the laying open the bladder into the rectum, or any other additional operation for the removal of a large stone. But I was under the necessity of delaying till I should be provided with the means of overcoming the difficulty. At this time, too, I was informed by Mr. Clover, who administered chloroform, that the patient was becoming weak. Removed to bed, he speedily recovered. In the evening, he was, to all appearance, and to his own feeling, quite as well as in the morning. He was now again submitted to the action of the anæsthetic agent; and, while he still lay in his bed, I applied the instrument represented in the accompanying figure, and by means of it the stone was removed. It was not directly broken by the force of the screw, but it fell into two parts when this was loosened.

On the morning after the operation, the patient was in a favourable state. He called for tea and toast for breakfast. During the after part of the day he vomited, and vomiting occurred repeatedly on the second day till evening, when it ceased, after the bowels had acted under the influence of an enema and he had taken a pill of opium.

Jan. 12. Fourth day: he slept well and calmly during the last two nights. Suffers at times pain in abdomen; but this is relieved by the passage of flatus, which he has long been very subject to. He lies on his side usually. The countenance is natural; abdomen not tumid. He takes liquid food in small quantities, and at short intervals. There is nothing unhealthy in the condition of the wound. The urine escapes freely, and it is quite clear. This has been the case since the first morning after the operation.

There were, it will be inferred from the foregoing statement, two sources of difficulty in the case, namely, the enlargement of the prostate, and the large size of the stone. The large size of the latter, it is well known, is also a serious addition to the danger of an operation. The influence of increased size of the gland, which does not add to the danger as it does to the difficulty of the operation, may be illustrated by reference to an instance that occurred in the practice of Mr. Liston in the same hospital, and I will refer to it in the words of the late Mr. Samuel Cooper, who witnessed the operation. The operation was, says Mr. Cooper in the 'Dictionary,' "excessively long, and a portion of the enlarged gland which had been sliced off fell down upon the floor." This, it is to be remembered, is the language of a looker-on, who, as is apparent from the remainder of the passage quoted from, was practically unacquainted with the difficulty of such a case. As regards the portion of the prostate alluded to, it was removed, I believe, for I was present at the time, by the pressure of the stone upon the gland after this had been notched twice at one side to give increased room. The stone was not in this instance a large one. But the passage is here cited to show the difficulty the enlarged gland creates in the operation, with even so dexterous an operator as Mr. Liston is well known to have been.



As regards the instrument with which the stone was removed, a few words of explanation are necessary. The blades were passed separately, the bent one first. The arrival of this behind the calculus was well marked by the jerk which occurred when its point left the side of the foreign body. It is a useful part of the construction of the instrument, that the pivot which serves as a hinge may be fixed at various distances from the end of the blade. The

action of the screw on the handles would, I have little doubt, be sufficient to break most calculi. As a forceps, in addition to other obvious advantages, it occupies less space in the wound than any ordinary one that could be applied with effect to remove a large calculus. For the instrument I am indebted to Mr. Weiss.—*Med. Times, Jan.* 17, *p.* 58.

108.—*Case of Stone in the Bladder.* By F. A. BULLEY, Esq., Surgeon to the Royal Berkshire Hospital.—[In this case William Evans, a labouring man, was admitted October 28, 1851. He had not passed blood with his urine, nor had he any of those painful sensations at the extremity of the penis, usually accompanying the presence of stone in the bladder. The presence of a calculus, however, had been clearly ascertained previous to his admission into the hospital.]

Nov. 14. The patient being now comparatively free from constitutional disturbance, and otherwise in a favourable state to undergo the operation, lithotomy was performed in the ordinary manner; the incision of the integument and muscular tissues of the perinæum being free, with a limited division of the prostate gland, which was partly divided with Fergusson's probe-pointed bistoury, and partly with the blunt gorget, on a staff of French construction, with a very deep and wide groove placed, not laterally, but at the back of the instrument. Nothing particular occurred during the operation, besides the division of a small artery, which afforded a trifling hæmorrhage, but did not require a ligature. The stone, although the incision of the prostate had purposely been made very small, was easily laid hold of and extracted; it was of a somewhat quadrangular flattened form, weighed an ounce and two scruples, troy, and measured an inch and 5-8ths in its length and breadth; not particularly rough upon the surface, and was apparently composed entirely of lithic acid.

From this date, the patient gradually recovered from the effects of the operation without the occurrence of a single unfavourable symptom; the urine continuing to flow through the wound until the 13th day, when the whole of it passed through the urethra; all the former irritation of the bladder has subsided, and the wound in the perinæum has healed; he will therefore be shortly discharged from the hospital.

Remarks.—I should scarcely have considered this case worthy of being recorded, except that I thought it went to illustrate a fact of great practical importance to the successful performance and issue of the operation of lithotomy; I allude to the dilatability of the prostate gland—a circumstance which has been dwelt upon with great earnestness by several distinguished writers on the subject, especially by Sir Benjamin Brodie, and more recently by Mr. John Adams in the last chapter of his valuable work on "The Pathology of the Prostate Gland."

Thus, in the foregoing instance, I purposely made a very small incision of the prostate, not pushing the blunt gorget so far as it is usually pushed in the operation, but only just so far as to allow of the fore-finger of the left hand being passed into the bladder, where it appeared to be grasped by the divided prostate so tightly as only just to allow of the introduction of the flat blades of a small pair of forceps by its side, when, having grasped the stone, which it will be seen was of considerable size, and greatly disproportionate to the limited dimensions of the incision, I succeeded, by two or three gentle lateral movements of the instrument, in extracting it, no impediment being offered by the perinæal muscular tissues, which had been freely divided in the operation.—*Med. Times and Gazette, Jan.* 3, 1852, *p.* 17.

109.—OPERATIONS FOR RETENTION OF URINE OCCASIONED BY INVETERATE STRICTURE.

By John Simon, Esq., F.R.S., &c.

[If a patient from stricture of the urethra is unable to empty his bladder, and all attempts to effect this by the catheter and other appropriate means have failed, how are we to give an artificial vent to the urine?]

The necessity for making an artificial opening into the urinary passages may be established, for such cases as we are considering, under any one of these three conditions, viz.:—

(1) All means for procuring the natural discharge of urine may absolutely fail; (2) urgent constitutional distress may render it dangerous to temporise, as in attempting the gradual dilatation of the stricture; (3) the urethra may have ulcerated at the seat of stricture, and may be allowing extravasation of urine to occur, with its attendant wide-spread mischief.

1. As regards the first condition, I believe it to be of the very utmost rarity. Taken simply and singly, it has never yet driven me to the necessity of operating. The local state of stricture which determines complete retention of urine is a compound one. Though the canal be permanently so small as to make urination very laborious, perhaps allowing the patient to effect it only *guttatim;* yet that which brings him to a dead lock is a temporary work. The mucous membrane is swollen by some additional congestion of blood, or the canal is obliterated by muscular spasm. And over this temporary aggravation we have great control. A full dose of opium,—aided, perhaps, by leeches to the perinæum and by the hot bath, or in some cases preceded by the action of a brisk purgative, will generally give relief; and thus, even if we cannot extemporaneously get a catheter into the bladder, we can re-establish the patient in his previous state of dribbling urination; we can insure that his bladder shall partially evacuate its contents; and we can gain time for that gradual dilatation of the stricture which will bring more complete and permanent advantage. With these resources in your hands, and with an expert—but, above all, a gentle and patient management of the catheter, I can promise that you will scarcely ever find yourselves defeated in uncomplicated cases of retention of urine from stricture. Should that rare contingency arise; should your milder measures utterly fail; should the urethra remain absolutely impervious—letting in no catheter, letting out no urine; then undoubtedly, as I have stated, this condition would establish a necessity—a legitimate, and imperative, and urgent necessity—for your making an artificial vent for the distended bladder.

2. The second specified condition for the performance of such an operation is a more frequent motive than the first. The stricture, though very close, may not be quite impervious; it may let enough urine pass to keep the bladder free from fatal distension; it may even (though this would be unusual) permit your smallest catheter to traverse it; yet with all this, your patient may be dying. He is an old man, perhaps, with a shattered constitution; he has been plagued with his stricture for years; it has been neglected or aggravated; his urine is fetid and full of pus; he has had constantly recurring rigors; his loins are painful and tender; every attempt at dilatation of his urethra gives severe suffering; his shiverings and sweatings have left him each day feebler; his weak pulse beats above 100 in the minute; his hands are tremulous; his tongue is getting dry; he is threatening to become typhoid,—breaking down under the prolonged irritation of his local disease. You may entertain no doubt that, with time, you could dilate his stricture; but here exactly it is, that time is an ally you cannot reckon on. A fortnight or three weeks would be requisite for your endeavour to have any success; and far within that period you would have nothing but the dead body to catheterise. Here, obviously, there is urgent need for immediate and complete relief,—for relief that shall at once put the man into a tranquil and painless state; and the establishment of an artificial outlet for his urine is the clear indication of treatment.

3. The third condition which I have stated to warrant the necessity of this proceeding, is that under which we most commonly adopt it,—where, namely, the uretha has given way behind the seat of stricture, and presents an aperture through which, at every contraction of the bladder, urine is effused amid the adjacent tex*tures, exciting them to* inflammation and gangrene. I need hardly tell you, that

cases of this description are among the most urgent in surgical practice, and that the utmost promptitude of relief is requisite for the patient's safety. Though the stricture, so far as that goes, may be of a kind likely to yield to gradual dilatation, yet, pending this slow process, what is to become of the urine? Is it to continue its destructive course of effusion amid living textures? Manifestly not; and therefore the local treatment resolves itself under two heads: first, to adopt such a course, relatively to the strictured and perforated canal, as will prevent any further extravasation of urine; secondly, to make such incisions as may be requisite for discharging out of the infiltrated tissues all their fetid accumulation of urine, pus, and sloughs. In seeking to fulfil the former of these indications, we find it necessary (as under the conditions previously considered) to make an artificial passage for the urine; and the operation, as I have said, has its most frequent necessity in the condition here adverted to.

And now, suppose the necessity be established for your giving an artificial passage to the urine;—suppose one of the three conditions to be present which I have stated to you; that you cannot procure any discharge whatever by the natural channel; or that your patient is suffering urgent constitutional distress from the insufficiency of such relief as you have procured him; or that the perinæum and genitals are beginning to swell with extravasation of urine;—now, what course have you to adopt?

The operation which for a great many years has been prevalent here (and, indeed, in most London schools of surgery) has been the following:—(1) A catheter or sound has been passed up to the seat of stricture, at, or near the bulb of the urethra; (2) a long cut has been made in the raphe of the perinæum, reaching down to the urethra, and opening it behind the seat of stricture; (3) the urethral incision has been prolonged forwards, towards the point of the catheter or sound, so as to split open the contracted portion of the canal; and finally, (4) all impediments being overcome, a large catheter has been conducted along the urethra into the bladder, and there secured by appropriate bandages.

You will find this operation fully described in Mr. South's translation of Chelius, and spoken of as the practice of the Borough hospitals for the last thirty or forty years. And, if you wish to see the operation in its most favourable aspect, you cannot do better than observe it in Mr. South's hands, who has had great experience in the proceeding, and who executes it with all the care and patience which are indispensable for its success.

Prof. Syme, of Edinburgh, who is a great authority in such matters, speaks of this operation as "protracted, uncertain, dangerous, and unsatisfactory." Looking to its average performance, I must say my experience would justify this censure. It is *protracted*, for the patient undergoes severe manipulation during a period, of which the mean would be twenty to thirty minutes. It is *uncertain*, for the division of a stricture or strictures to which one is so imperfectly guided, cannot be accomplished with facility: nor can one feel sure, under the most favourable circumstances, that one's scalpel has hit the exact line of a canal contracted (perhaps for an inch of its length) to such narrow dimensions as scarcely, if at all, to admit the smallest catheter.* It is *dangerous*, because (in addition to the sources of risk just adverted to), large hemorrhage not unfrequently contributes to exhaust the patient; and further, because in many cases (as where the urethra is contracted throughout its whole spongy portion) a catheter cannot be maintained in the passage, without prolonging that state of pain and irritation which already have set life in jeopardy. And *unsatisfactory* it must be on all these grounds. For what can be more so, than to conclude a severe and dangerous operation with uncertainty, as to whether one has accomplished that very object for which the severity and the danger were encountered?

These objections apply to the proceeding as practised by the best (I mean the most careful) operators. In other than good hands, it is a very horrid affair: metallic instruments are thrust in all directions; they leave the canal at one place, and re-enter it, by perforation, at another; or they pass up to the hilt—one shud-

* "Even under the most favourable circumstances, it cannot be otherwise than doubtful whether the stricture be properly divided; that is, whether the incision has passed through the narrow canal in the centre, or through the solid substance on one side.—*Brodie on Diseases of Urinary Organs*, p. 96.

ders to think where!—and draw no water; the rectum, the prostate, even the bladder undergoes injury in these violent efforts, and the patient is eventually sent to bed, it may be with his bladder unemptied, having his chance of cure sensibly diminished by the infliction of so much unnecessary mischief.

We cannot be surprised that many surgeons have taken refuge from the precarious chances of this operation, in the comparatively simple and secure process of tapping the bladder by the rectum or above the pubes. I shall presently describe to you the operation which, generally speaking, I consider a far preferable alternative even to these. But, before examining their comparative merits, I have still something more to say in respect of the last.

Within these few years, Professor Syme has introduced a method of dealing with obstinate strictures, by dividing them on a director previously passed through the constriction; and you may ask whether the adoption of this manœuvre would be applicable to the cases we are considering, so as to remove the objections I have expressed to the operation of dividing the stricture without any such assistance? I think not. The cases heretofore treated by Mr. Syme's operation have not been cases where the primary consideration is to give immediate relief to a distended bladder, or to provide against advancing extravasation of urine; and in cases such as these there are generally circumstances which would render the director inadmissible. If the urethra is impervious to a small catheter, it is not likely to yield to this other instrument; if the canal has ulcerated, so as to communicate with infiltrated and sloughing tissues, or is riddled with false passages, the director would be not unlikely to prove a treacherous guide. Here and there one might find a case in which (supposing division of the stricture to be our desideratum) Mr. Syme's principle would admit of application; but, speaking generally, I may repeat that his proceeding relates to quite a different class of cases.

But, if that "protracted, uncertain, dangerous and unsatisfactory" operation, which I have described, could admit of serviceable modification by Mr. Syme's proceedings, there is yet another reason, I think, which would induce us to reject it.

The operation held its ground because of its alleged completeness. The notion of dividing (and therein *curing*) the stricture, at the same moment as one gave relief to the distended bladder, was indeed charming. But, of late years, surgeons have discovered, that this seductive completeness had in it a practical fallacy. The division of the stricture was the sheerest superfluity. Let the bladder be relieved any how,—by the perinæum, by the rectum, by the pubes; merely let the stricture for a while be undisturbed by the constant irritation of urine urged against it from behind, and there speedily occurs a spontaneous perviability of the canal. The stricture wants no cutting. It loosens itself.

So remarkably does this effect belong to the withdrawal of pressure from the stricture, that it arises, not only when the surgeon has made an artificial outlet for the water, but also under far less favourable circumstances; namely, where the urethra has given way behind the stricture, and where the bladder expends its chief expulsive force in driving the urine into surrounding textures. "The first effect of this mischief (says Sir Benjamin Brodie) is to relieve the patient's sufferings; there is no more straining, and the spasm of the stricture, no longer excited by the pressure behind, becomes relaxed, so as to allow some of the urine to flow by the natural channel." Further, in the very numerous cases where the endeavour to divide a stricture has been defeated by the difficulties I have adverted to, and where (contrary to the performer's intention) the operation has not advanced further than the stage of cutting into the bladder or urethra somewhere behind the obstruction, the same loosening of the stricture has been observed to ensue, as though it had actually been divided.

Surely, it cannot be desirable to incur difficulties and dangers in attempting to divide a stricture, when the same advantages spontaneously arise without that division being accomplished.

These considerations have led me to the modified perinæal operation, which you have seen me perform in various instances, and which consists simply in this: I open the urethra by puncture (or by very small incision) immediately in front of the prostate gland. I run a short elastic catheter along this wound to the bladder. I leave the stricture quite untouched for ten days (more or less) during which the *urine flows entirely* by the perinæal catheter. At the end of this time, I find the

stricture sufficiently relaxed for me to begin its dilatation with a middle-sized instrument; and I thus obtain all the advantages assigned to the severer and more difficult measure, while adopting an operation of extreme slightness and security.

I have already intimated to you, that many surgeons, feeling the risks and difficulties which are inseparable from the ordinary perinæal operation, have chosen rather to tap the bladder, in such cases as we are considering, either above the pubes or by the rectum. Obviously, on the principles which I have stated to you, either of these proceedings might give very satisfactory results. The bladder would be effectually emptied, and the stricture relieved from irritation; supposing the urethra to have given way, diffusion of urine would be prevented; and neither of these operations can be considered very difficult or very dangerous.

If, therefore, my choice lay between the ordinary perinæal operation and these other expedients, I should not hesitate to prefer one of the latter. But the modified perinæal operation which I advocate is still simpler and safer.

[Though a man of ordinary skill could scarcely bungle in tapping the bladder through the rectum, yet it requires some practice and dexterity. The peritonæum is not always so safe from injury as some advocates for the operation believe, in fact it is somewhat of a roundabout operation, to involve a second viscus to relieve the first; and if we could relieve the bladder by cutting through the common integuments it would be as well to leave the rectum alone. But the supra-pubic puncture is not advisable, although we may sometimes be necessitated to resort to it. Mr. Simon proceeds:]

Reviewing the objections I have briefly stated, I cannot but give a decided preference to the modified perinæal operation, in all cases which admit its execution. The point of the urethra selected for the puncture is definite in its position. It is readily reached from the surface of the perinæum. No important parts intervene. The subsequent escape of urine is direct. The position of the catheter causes little inconvenience. The perinæal incision necessary for reaching the urethra is in nearly all cases required by accumulations of pus and extravasated urine. In such cases nothing is wanting to relieve the bladder but to deepen this incision into the urethra itself—a proceeding surely both milder and more obvious than if, after cutting deeply into the perinæum for pus and extravasated urine, one were to start *de novo* with a trocar, to tap the bladder by the rectum or above the pubes.

[Mr. Simon gives six cases, in all of which the operation,—the modified perinæal one,—was quite successful. The first case is given fully.]

Case I. J. W., a lighterman, aged 55, of damaged general health, was admitted into Abraham ward, January 22nd, 1850. He had suffered from stricture for sixteen years; and during the last three years had had a fistulous opening in the scrotum. Urine was discharged in the smallest stream, and only with extreme effort. His urethra was a string of strictures from end to end. Attempts were made twice a week to effect their gradual dilatation by bougies; but much suffering was caused, and little progress made. He had frequent rigors; his nights were restless; and he had little or no appetite. February 14th. The painfulness and sensibility of his urethra made it impossible to persevere in the use of instruments; and, though he was still passing urine, his quick, powerless pulse, anxious countenance, and dry, brownish tongue, showed that he was under great constitutional distress.

The following day, at noon, when I saw him, he was worse; the pulse at his wrist was but just perceptible; his voice was barely audible; he was verging on the typhoid condition, and apparently had but few hours to live. Any severe operation was obviously inadmissible; I would not even let him be carried to the operating theatre, but had him placed on the table of the ward, secured in the lithotomy position. With a single puncture I guided my bistoury into the membranous portion of his urethra, immediately in front of the prostate: and then, as the urine flowed, carried a large elastic catheter along the wound into his bladder. He was immediately replaced in bed, and stimulants were given him liberally and frequently. His relief was immediate; and though for many days he continued in a most precarious condition (his life being additionally endangered by an attack of bronchitis), yet we succeeded in keeping him up, and, eventually, in restoring him to health. After a fortnight, the elastic catheter was withdrawn, and gradual dilatation of his strictures begun. He remained for a long while in the hospital, rather

for his general health than on account of the local disease; and when he left, could retain his water as long as desirable, and discharge it in a full stream.

Several months afterwards he came under my care again, on account of severe ophthalmia; I was then enabled to learn that he was quite free from inconvenience in his urinary organs.

[Mr. Simon believes that in these cases no other operation could have been equally successful. He goes on to remark:]

In the first case the patient was moribund. I do not know that I have ever seen a man with chronic urinary disease recover from such apparent proximity to death. If I had set about dividing his strictures in the perinæum, and forcing a large catheter along the contracted channel of his penis, the loss of blood and the protracted pain would have been too much for him. He would probably have died on the table. It is true that his urgent symptoms would have been relieved if I had tapped the bladder above the pubes, or by the rectum; and his case, in my judgment, is that one of the series in which it most nearly became a matter of indifference which of these operations should be performed. I have already told you the general principles on which I consider the perinæal puncture preferable to either of the other operations; and in this case, as generally, I saw no reason to choose the greater risk of the supra-pubic puncture, or to effect, by stabbing through the bowel, what could equally be done by traversing the skin. Most effectual relief was given him at a minimum expense of injury, and this, it need not be urged, is the desideratum in every surgical operation.

In all the other cases, urine was actually tending to the perinæum. An opening of the urethra (though an insufficient one) had been established by an ulcerative process behind the seat of stricture. A perinæal incision was indispensable for the evacuation of pus and extravasated urine. Under these circumstances, to make a second wound through the rectum would have been the infliction of unnecessary mischief.

Indeed, in such cases, one hesitates whether it might not be expedient to advance a step further in the process of simplification; and, since nature has bored a hole in the urethra, whether we might not remain satisfied with a free division of the superficial parts, trusting to the sufficiency of the ulcerated hole (unassisted by a catheter) for maintaining the bladder without distension, and the stricture without irritation.

My one experiment in this direction was not satisfactory. In this instance (*Case* 5) as the urethra was pervious to a small catheter, and as no considerable diffusion of urine was in progress, I contented myself with a free perinæal incision, reaching to the urethra; and I trusted to the urine finding its way readily by this channel. But on the morrow it was clear that this measure had been insufficient; urine was apparently making its way, according to the general course of its extravasation, towards the pubes; and I therefore, with marked advantage to the patient, passed my bistoury a second time into the wound, opened the urethra there, introduced an elastic catheter, and thus diverted the entire stream of urine away from the parts where it was hurtful. Rationally, too, one would expect this to be, for all such cases, the safer and better course. One cannot be sure of the exact spot where the pipe has given way; the ulcerated opening may be on one side or the other; it may even be in the upper wall of the urethra; so that its communication with the perinæal vent would be oblique or circuitous; it may be so restricted or valvulated by fascia, as to give the urine facilities for creeping preferably in other directions. It is, I think, an indication of supreme importance, to make the artificial channel of escape as direct and as free as possible.

The fourth case was peculiar in respect of the aperture already made into the rectum, through which urine was flowing; and when the circumstances were under consideration, a very obvious thought arose—whether that communication, already existing between the bladder and rectum, could be turned to account for the patient's cure. I determined in the negative. In its actual state it was evidently insufficient; for extravasation of urine was advancing to a frightful extent. It would have required surgical assistance (by trocar or otherwise) to make it available: and any such proceeding would have been made difficult and hazardous by the abscess between the two viscera, and by whatever change in their mutual relations *that abscess had* occasioned. If the rectal opening of the abscess were to

remain plugged by a canula, who knew but we might have trouble from distension of the suppurating cavity, leading perhaps to some distant and less convenient discharge of its contents, or attended by increase of irritation in those inflamed tissues which the canula must traverse? Looking to the patient's almost typhoid condition, I dared not turn a hair's-breadth from the course which would give him, I knew, instantaneous and complete relief; so, while evacuating by an incision the fetid accumulation of pus and urine with which his perinæum was distended, I likewise punctured his urethra in the manner I have described to you, and conducted a catheter through the wound. Nothing could be more pleasing than his after-progress; he was immediately at ease. One could see how immense a weight had ceased to press down the springs of his life; and in a very few hours he was removed from imminent peril of death to a state of comparative security.

Thus much for the cases; and, as regards the mechanical details of the operation, I need scarcely add to what I have already said, beyond recommending you to practise on the dead body, at every convenient opportunity, the art of reaching the urethra at its membranous portion without the guidance of a staff. On your power of doing this depends your right to attempt the operation I have described to you. But what can be easier? The canal which you wish to penetrate is not a small one; often, indeed, it is considerably dilated in consequence of the diseased condition which obliges you to operate; its position is invariable, and in every point of its course can be readily explored from the surface. The bulb is subcutaneous. The prostate you feel within the anus. The length of the canal between these two points is not an inch; its course straight in the median plane. Any difficulty which might be occasioned by the bulging of the perinæum with pus or extravasated urine ceases, of course, with the first incision, which (in such cases made with proper freedom) gives immediate vent to the confined fluid, and enables you to proceed with facility. Often in thin subjects, and where the urethra has not given way, the distension of this canal, as the patient strains to make water, will render it so evident, that your operation may resolve itself into a mere puncture with a lancet. In the less easy cases, where your subject is fat, or the perinæum deep and infiltrated, there is really nothing to deserve the name of difficulty. You make a sufficient cut in the raphe, terminating a little in front of the anus, and sinking as deep as may be requisite into the cellular tissue. You may then, in the following way, arrive at the point of the urethra which you wish to penetrate. Pass your right fore-finger into the anus; ascertain, through the wall of the bowel, the position of the prostate; bring your finger forward till it discovers the anterior extremity or apex of the gland; let it just pass this spot, and rest (nail upwards) pressing with its point immediately in front of the gland. Of course, if the parts were transparent, you would now see your finger indenting the membranous portion of the urethra at that hindermost point of its course where you purpose to puncture it. Now pass your left fore-finger (nail upwards) into the wound; advance it till (with the guidance of the finger in the rectum) it falls against the apex of the prostate; there you so arrange it, that the middle phalanx presses back the rectum; the last phalanx lies along the prostate, with the tip of its nail indicating the spot at which the urethra emerges. Finally, withdrawing your right fore-finger from the anus, and resuming the bistoury, you run this along the left fore-finger, till you penetrate the canal on which it rests, and immediately follow it by the short elastic catheter which you intend leaving in the bladder.

The cut is made into the urethra, you observe, just at the confines of its prostatic and membranous portions; a spot which is posterior to the seat of stricture, and is easy to hit, from the definiteness and invariability of its position.

With a little practice on the dead subject, you will readily acquire the knack of doing this operation in the natural condition of the parts with a single puncture; and you will find that disease alters those natural relations far less than is commonly stated. But in the most difficult cases which can come before you, if you follow the rule I have given you, and carefully determine through the rectum the exact point at which the urethra emerges, you will fail to find any embarrassment, and will complete the operation in much less time than I have taken to describe it.

The after-treatment of these cases, for some days succeeding the operation, is not unimportant. Liberal allowance of stimulants is often required, sometimes from the very first. This purpose I generally effect by wine, or (if the stomach be

irritable) by brandy with soda water. Actual drugs I rarely use, unless it be to procure action from the bowels, which, if their secretions be much disordered, I do as early as possible, with either colocynth or compound rhubarb-pill, in combination with blue pill. Opium I do not find admissible.

As regards the history of the operation which I have recommended to you, I cannot give you very full details. If you refer to Sir Astley Cooper's Lectures on Surgery, you will find that, at the time of their delivery, he recommended in cases of simple stricture, that a puncture should be made into the urethra where distended by urine, immediately behind the seat of stricture; and Sir Benjamin Brodie (who rather leans to the rectal operation) speaks of the puncture of the urethra as "a sufficiently simple and unobjectionable proceeding." As far as I can judge from Sir Astley Cooper's scanty description, it was only in cases of stricture far forward in the urethra that he adopted this course, and "passed a lancet" into some part of the canal anterior to the bulb. At least, if he ever practised any such operation as I advise for strictures situated further back in the urethra, so that his puncture would have been made in the vicinity of the prostate, I suspect that he soon afterwards abandoned it for the supposed advantages of dividing the stricture. The latter operation seems to have taken its rise about forty years ago. It was first practised, I believe, by the late Mr. Grainger, of Birmingham (father of my distinguished colleague, our teacher of Physiology), and is very well described in a volume of 'Medical and Surgical Remarks,' published by that gentleman in 1815. It soon became the general operation for cases of stricture, and has been extensively practised, in the borough hospitals and elsewhere, down to the present time. When Mr. Grainger found himself unable to accomplish it, he used to make a partial division of the prostate, as in the lateral operation of lithotomy, and thus convey an elastic catheter to the bladder.

Sir Benjamin Brodie recommends, in cases where the urethra has given way behind a stricture, and where a bougie can be introduced, that this should be used as a director for the introduction of a perinæal catheter, and that the latter should be left in the wound for one or two days.

I am not aware of any surgeon having habitually practised the operation in the form I have described; and, from such observations as I have made on the subject, it seems to me well worthy of more general adoption. Practised in the manner I advise, it may, I think, entirely supersede the operations for tapping the bladder, except in those very rare cases of prostatic tumour, where the supra-pubic puncture is inevitable. It likewise entirely annuls the supposed necessity, while it avoids the difficulties and dangers, of dividing the stricture *in perineo* for the relief of retention of urine. And its advantages, meanwhile, are purchased by so trifling an endurance of pain, inconvenience, or injury, that I could not cite to you, from the whole practice of surgery, any parallel instance of disproportion between means and results—any instance where, from an extremity of disease, suffering, and danger, the patient is suddenly removed, by surgical appliances so simple and so secure, to a condition of comparative enjoyment and safety.

The only argument likely to be urged against the proceeding in question is one which I may best anticipate and answer in Sir Astley Cooper's words. "This operation has been objected to (he says) on the supposition that it requires great anatomical knowledge. To this objection I will say, that he who is adverse to an operation because it requires anatomical knowledge, should immediately give up his profession; for if surgery be not founded upon an accurate knowledge of anatomy, it will be better for mankind that there should be no surgery, as disease will proceed better with the natural means of relief than with the aid of those surgeons who are not anatomists."—*Med. Times and Gazette, April* 10, *p.* 359, *and April* 17, 1852, *p.* 384.

110.—*Puncture of the Bladder by the Rectum for Retention of Urine.* Under the care of EDWARD COCK, Esq., at Guy's Hospital.—[S. L——, aged 43, was admitted Jan. 16, 1852, in the evening. The patient was suffering great distress from distension. He had been subject to stricture of the urethra for the last ten years, after repeated attacks of gonorrhœa. Had been relieved many times previously by *means of the catheter*, though with difficulty.]

On admission, the bladder appeared very much distended, reaching nearly as high as the umbilicus, and the patient was suffering much. All attempts to pass an instrument into the bladder by the urethra having failed, the man was ordered one grain of opium and one of calomel, to be taken immediately.

On the next day the patient was somewhat relieved; he had perspired profusely during the night, and a small quantity of urine had escaped guttatim by the urethra. The catheter was again tried, but without success. Mr. Cock, therefore, punctured the bladder by the rectum with a trocar and canula, and drew off three pints of ammoniacal urine, much to the relief of the patient. The canula was then secured by the inner blunt-edged tubes being introduced within it, and the whole was left in the bladder.

The man went on well for the first week. The canula remained in the bladder, but the inner tubes were occasionally removed, cleaned, and re-introduced.

On the twelfth day the canula was removed, as he was suffering slightly from the irritation produced by its presence in the bladder.

On the thirteenth day, the right testicle became inflamed and swollen, and a small abscess appeared in the perinæum. Both these symptoms yielded easily to treatment, and were not at all connected with the urinary canal.

Fourteenth day.—The patient now passes a minute quantity of water by the urethra, but no instrument can be guided through the seat of stricture.

Thirty-fifth day.—A No. 1 catheter was introduced into his bladder for the first time since the operation, and was secured, the patient having passed nearly the whole of his urine by the artificial opening in the rectum for five weeks.

Forty-first day.—The urethra is daily gradually dilated, and now a No. 5 elastic catheter can be passed without any difficulty.

Since the introduction of a catheter into the bladder, not any of the urine has passed by the bowel.

March 1st, being the forty-fourth day after admission.—The patient is now well, and able to pass a good-sized stream of urine by the urethra, and the puncture in the rectum has entirely closed.—*Lancet, April* 3, 1852, *p.* 331.

111.—ON THE PROMPT REMOVAL OF STRICTURES OF THE URINARY CANAL WITH THE URETHRAL GUIDE AND TUBES.

By Thomas Wakley, Esq., Surgeon to the Royal Free Hospital.

[Mr. Wakley remarks that the introduction of the smallest guide into the bladder insures the passing, by degrees, of sliding-tubes of the largest size through the strictured portion or portions of the urethra; the guide rendering it impossible for these tubes to take a wrong course, and the effect of their introduction being to afford immediate relief to the patient.]

It will be found that the perfect command of the urethra, by means of the guide and metallic and elastic tubes, is a source of immense satisfaction, and of inestimable practical value. The metallic tubes having cleared the passage and having stimulated the absorbents into action, the elastic tubes come into operation, and secure the most favourable condition of the canal for the completion of the curative process. In all the varieties of stricture that have fallen under my observation, these simple and safe instruments* have proved of advantage. In the mildest as well as in the severest forms of the disease, they may be employed with equally good results and freedom from danger. In only two instances have any troublesome constitutional symptoms arisen subsequent to their employment, and in not one case where I have had an opportunity of *repeating* the attempts to pass the guide through the stricture into the bladder, has there been a failure. Frequently, such is the twisted and distorted state, such the false passages of the canal, that much patience is required in order to effect the introduction of the guide. This operation being accomplished, the first, and it may almost be said, the only difficulty in the treatment is surmounted, and henceforth the *free passage* of the canal is at the command of the operator.

* See Retrospect Vol. xxiii. for engravings of these instruments.

It affords me much gratification to quote from the valuable work of Mr. Guthrie, his remarks in favour of the new instruments. He says,—

"This may be accomplished (removal of the obstruction) after the manner lately recommended by Mr. Thomas Wakley, and it is impossible to speak too highly of this invention. . . . . It is capable of rendering great service when the withdrawal of a sound or a catheter cannot always be certainly followed by the re-introduction of another, and which withdrawal it renders unnecessary, until a larger one is introduced over it—a very great improvement which no surgeon should neglect; for where this can be done, no operation is immediately necessary."

This testimony, from so distinguished a surgeon, is highly appreciated by me, and will be duly valued by the profession.

The reports of cases now in my possession, embrace nearly all varieties of stricture of the urethra that are seen in the hospitals of this metropolis. Selections from them, commencing with the simple forms, and concluding with the most complicated and obstinate, will, I think, illustrate very satisfactorily the successful action of the urethral guide and tubes. It will be remarked that the reports refer to strictures with lengthened, cartilaginous, and riband-like bands; false passages and sinuses; urinary fistulæ and retention of urine, either partial or complete; and indeed almost every complication calculated to embarrass the practitioner.

Ample experience justifies me in stating that during treatment for the cure of permanent stricture, the state of the patient's health demands the unremitting attention of the surgeon. The normal action of the digestive organs and intestinal canal is essential in many cases to the speedy and easy relief of the patients. Occasionally the constituent qualities of the urine should be ascertained, as highly-irritating urine frequently tends to impede the progress of the cure, by causing spasm and inflammation of the newly excited parts of the urethra. This is a subject of considerable importance, and it cannot well receive too much attention in the early periods of the treatment. Sometimes, in very severe cases, it is necessary to administer opiates, but examples of this description are very rare, provided the tubes are not rashly and violently introduced. The local treatment consists of poultices, fomentations, or leeches, or of all three, as the symptoms may indicate. When there is much cartilaginous thickening of the strictured part, and also tenderness on pressure, poultices applied to the perinæum on the first two or three nights after the introduction of the tubes, are productive of great advantage. It may seem to be superfluous to point out the necessity of resorting to such simple agents in the treatment of strictures of the urethra; but I entertain a totally different view of the subject. A system of treatment that fails under the direction of one surgeon succeeds under the guidance of another, because the latter attends to circumstances which the former altogether disregards or neglects. The practitioner who omits to devote attention to any of the facts or conditions which are strictly characteristic of the disease under consideration, or of the treatment adopted for its cure, must, in my opinion, fail in the performance of an essential part of his duty.

[In the case of a carpenter, the disease had been occasioned by a severe attack of gonorrhœa, which had been allowed to wear itself away without the adoption of any kind of treatment. A slight gleet had continued for the last ten years.]

On examination, it was found that a stricture existed at about five inches from the meatus, and that it was cartilaginous. After some little difficulty, I succeeded in introducing the guide, and then the tubes up to No. 4. The operation was comparatively painless. After No. 2 had been passed, Nos. 3 and 4 entered the strictured part with comparative ease. There was no hemorrhage. At the third visit of the patient, I introduced a No. 10 tube over the guide, and at the fourth visit, a No. 10 common sound passed through the urethra with perfect facility. In this case the great advantage of being enabled to increase the size of the instrument without losing the command of the urethral canal, was very strongly manifested. Under the common system of treatment, it would have required weeks to have cured this disease.

E. H——, March 10, 1851, aged twenty-five, an ostler residing in Gray's Inn lane, applied at the hospital, having a very troublesome stricture, situated about four inches from the meatus. He states that the disease was caused by gonorrhœa *and intemperance*. In this case I had considerable difficulty in passing the catheter-

guide through the stricture, an operation which somewhat tried the patience of the gentlemen who were present on the occasion. At length, however, by cautiously continuing the efforts, the guide first, and then the metallic tubes, up to No. 5, were introduced completely through the stricture. Subsequently an elastic tube No. 4 was passed, and left in the canal for an hour. On the following day the operation was repeated, and metallic tubes Nos. 6 and 7 introduced. In less than three weeks this patient was discharged quite well. The absorption of the cartilaginous enlargement, in this instance, was very quickly effected, and the patient declared that he had not suffered from pain during the treatment. The improvement in the state of the general health of the patient was particularly rapid.

Richard B——, a tailor, aged forty-six, applied at the hospital, suffering from a very obstinate and intractable stricture of the urethra. It had existed seven or eight years. On passing the guide into the urethra, two distinct strictures were found. No false passages existed, although the contrary was alleged. The guide was easily passed through the first stricture, but the second, about an inch deeper in the canal, resisted for some minutes the efforts that were made to pass it. There was, in this instance, a sufficient amount of irritation to give rise to spasm. The metallic tubes were passed in rapid succession to No. 7, when a No. 6 elastic tube was passed and kept in the canal nearly half an hour. The operation was repeated daily and the tubes gradually increased. In eleven days the patient was discharged, perfectly well. The introduction of the guide at the first visit of the patient, rendered the case simple and easy without the appearance of a single constitutional symptom. In this case, six tubes of graduated sizes were introduced in one day, completely through the stricture. Of course they were all passed on the guide, which conducted them unerringly through the natural canal. In order to have obtained an equal degree of freedom in the urethra, the old plan of treatment would have required probably twenty distinct operations, and the difficulties first encountered twenty times repeated. The application of the tubes in this case was witnessed by many surgeons, amongst whom, on one occasion, I had the great satisfaction of seeing Mr. Guthrie.

John Y——, aged twenty-nine, a groom out of place, applied at the hospital, April 4th, 1852. He was suffering from stricture of four years' standing. He had great difficulty in voiding his urine, and the obstruction had increased very considerably lately. The urine is now discharged, with much pain, in a very small, thread-like stream.

On examination, I found a very firm stricture at the anterior part of the membranous portion of the urethra. After a trial of a few minutes' duration, the guide was introduced through the stricture, and then the silver tubes to No. 5 were passed. An elastic tube, No. 4, was then sent through the stricture, and kept in that position for about twenty minutes. On the following day, the metallic tubes were increased two sizes, and afterwards elastic tube, No. 6, was passed and retained for half an hour. This treatment was continued as in the other cases, and in a fortnight the patient could pass his urine in a full-sized stream, and was discharged cured. I saw this man in November, and he stated that he was quite well in all respects, that he was then in a situation as groom, and could perform all his duties as effectually as at any period of his life. His constitution, when he first applied at the hospital, was in a very shattered condition.

On April the 1st, Patrick D——, aged fifty-nine, a burly Irishman, applied at the Royal Free Hospital, with a recommendation addressed to me by my friend, Mr. Rawlins, surgeon, of Kentish-town, requesting that I would attend to the stricture. I examined the man, and found he had two strictures, one about two inches from the orifice, the other near the membranous portion of the urethra. There was considerable hardening in the region of the perinæum. The No. 1 catheter-guide could with difficulty be introduced; this accomplished, however, no further obstacle had to be surmounted. In fourteen days a No. 10 common sound could be passed with the utmost ease. At the end of three weeks this man was discharged in perfectly good health.

T. H——, aged sixty, a bricklayer's labourer, was admitted into the hospital March 6th, 1851, with an intractable cartilaginous stricture of the urethra, about six inches from the orifice. He stated that he had been under treatment in Ireland and different parts of this country at least twenty times during the last thirty years.

On introducing a No. 5 common sound, the instrument entered a false passage, about four inches from the meatus. A No. 2 guide, after an effort of about twenty minutes' duration, was passed through the stricture, and then onwards into the bladder, when a large quantity of offensive urine was discharged. The urethral tubes up to No. 5 were then introduced, and also an elastic tube. The operation was repeated daily, and the tubes gradually increased in size, without producing a single unfavourable symptom, or causing any pain. In fifteen days a No. 12 common sound was easily passed. This instrument I gave him at his own urgent request. He assured me that he would always pass it twice a week for the remainder of his life. I saw him about four months after his discharge from the hospital. The induration around the urethra had quite disappeared; the discharge had ceased; there was not any thickening of the passage remaining; his bladder and urethra acted perfectly, and his health had been so completely restored, that he had resumed his occupation, and stated that he could ascend the highest ladder with ease. From the history of this man's disease, it appeared that the false passage was the difficulty which resisted the ordinary mode of treatment. He said that at times, when he had been under treatment, he had "bled like a pig." The guide having been first introduced, the tubes were effectually prevented from entering the false passage, which was soon closed by the action excited in the part. This was a very interesting case.

C. C——, a butler, out of place, applied at the Royal Free Hospital, March 13th, 1851, suffering from stricture of the urethra. He passed his urine *guttatim*. He says that he has been examined at several hospitals, and by all pronounced to have an impermeable and incurable stricture, about seven inches down the canal. Has had a light, gleety discharge for years, following a very bad gonorrhœa which he contracted, and treated by powerful astringent injections. He appears much reduced by his disease—is desponding—thin. The small catheter-guide was passed into the bladder after considerable trouble, and a few drops of blood preceded the flow of urine. The metallic tubes were now used, up to No. 5—and the elastic tube No. 4 retained two hours. On the following day, the same instrument was used. The next day, the sizes were increased to 8, the elastic being retained two hours. In fourteen days from the commencement of the treatment, this hitherto intractable stricture yielded to the influence of the tubes, and a No. 12 sound then passed with ease. The man states that caustics had been frequently used, and always produced shivering, and very considerable hemorrhage. At the expiration of a month he was quite well.—*Lancet, Feb.* 7, 1852, *p.* 144.

112.—ON THE STRICTURE DILATOR IN THE TREATMENT OF STRICTURES OF THE URETHRA.

By Barnard Holt, Esq., Surgeon to the Westminster Hospital, etc.

[There is no doubt of the value of that instrument which, when being once introduced through the urethra, can be materially increased in size without any other instrument being passed in contact with the urethra. The instrument now under consideration is a modification of M. Perrève's, which Mr. Holt has termed the stricture dilator.]

The surgeon possesses, in this instrument, a means whereby dilatation can be carried to any extent; yet this advantage is not to be abused, inasmuch as many circumstances may arise, where extreme dilatation would be attended with great disadvantage. Thus, in cases where great spasm is present, the irritability of the urethra being evinced by persistent contractions and attempts to expel the instrument, the dilatation must be gradual, and only continued in obedience to the feelings of the patient. The rule of never using violence holds good here as elsewhere. In no case in which I have used the dilator, has there been anything further than slight smarting on the next attempt at micturition; no rigors, no retention of urine, or stricture fever, which, under the old system, so frequently attends the attempts to pass a second and a larger bougie, has resulted from its introduction. I cannot help anticipating its general adoption. It might be urged, that rapid dilatation is not so permanent as gradual; but this is surely a false argument, *inasmuch as having* by rapid dilatation gained a certain size of passage, that size

can be easily maintained, and certainly with less inconvenience than is occasioned by the protracted measures we are at present compelled to employ. Yet, were it only for one circumstance, I believe preference will be given to the dilator—namely, the facility of increasing the size, *without withdrawal of the original instrument*, and without any other being passed *in immediate contact with the urethra*—thus obviating the present necessity of a second and frequently unsuccessful attempt at introduction.

In many cases of old cartilaginous obstructions where the ordinary bougie accurately fits the diameter of the stricture, considerable pain is experienced in its withdrawal, from the tenacity with which the bougie is grasped by the stricture; and occasionally some amount of force is necessary to remove it. This difficulty is entirely obviated in the employment of the dilator, by the gradual and easy retraction of the tube, by which the blades of the dilator become immediately closed to the diameter at which it was originally introduced.

In conclusion, I venture to suggest that the stricture dilator is capable of effecting all that can be done by the ordinary bougie, and, in addition, has these advantages:

Firstly. The dilator, being introduced in a small compass, passes the stricture with greater facility and less pain to the patient.

Secondly. It can be increased from one to six, or six to twelve sizes, without removal from the bladder; thus entailing upon the patient only one introduction at each visit.

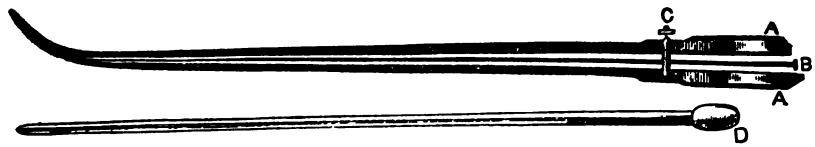
Thirdly. The means of increase is effected without more than one instrument coming in contact with the urethra.

Fourthly. There is no possibility of the dilating tubes lacerating the urethra, or escaping from between the blades of the dilator.

Fifthly. The dilatation can be increased, or relaxed, and accurately regulated according to the feelings of the patient, *without withdrawal of the original instrument.*

Sixthly. The increase is very gradual, whatever sized tube may be introduced, in consequence of the length of the upper and lower blades, and the conical form of the extremities of the tubes.

Seventhly. In the operation of lithotomy, it affords facilities for the introduction of a large and straight staff, by a slight modification of the form and size of the tubes.



A A, The upper and lower blades. *B*, The directing-rod.
C, The regulating screw. *D*, The dilating tube.

The Engraving gives but an imperfect idea of the *Dilator*, the curve being too short; and the directing-rod ought not to have any ornament at its extremity.

Description of the Dilator.—The dilator consists of an upper and lower grooved blade welded at the point, and fixed in a handle which admits of dilatation. Through its centre, or between the two blades, a wire or directing rod is fixed, upon which the tubes pass, in order to secure them from slipping from between the blades of the dilator. A screw is fixed in front of the handle, which regulates the size to which the instrument should be increased. The tubes are made of silver, and range from one to twelve, conical at their extremities, for the more gradual dilatation of the stricture, and in order that but little strain should be exercised on the extremity of the dilator. The dilator being once introduced, the screw should be partly removed, according to the sized tube it is to be desired should be used. The tube is then passed on the directing rod, between the blades of the dilator, and very gently pushed forwards, the dilator being firmly held in the left hand to prevent any jerking, by which unnecessary pain might be caused. The tube, having been gently passed to the extremity of the dilator, is to be permitted to remain until any smarting has subsided, when it may be withdrawn, and if necessary, a larger tube introduced—the surgeon exercising the same caution as before. In the generality of instances it is desirable that the tubes should not be

increased more than three sizes at one visit, otherwise constitutional symptoms might arise from the too violent pressure. The dilator being withdrawn, care should be taken that it is properly cleaned and wiped dry, as, were the directing rod to become rusty, some future inconvenience might be experienced in the passing of the tubes. The first dilator is capable of increasing the size from one to six, the second from six to twelve.

The dilator is made by Messrs. Philp and Whicker, of St. James's-street.—*Lancet, Feb.* 7, 1852, *p.* 146.

113.—ON THE MUSCULARITY OF THE URETHRA AND SPASMODIC STRICTURE.

By Henry Hancock, Esq., Surgeon to Charing Cross Hospital.

[The ignorance as to the causes of spasmodic stricture arose for a long time from the want of knowledge of the structure of the parts; for, until Mr. Hancock's investigations were made known, the muscularity of the urethra was ignored and denied. Although Mr. Hunter and Sir E. Home, from M. Bauer's researches, imagined muscular fibres to exist, yet both Sir E. Home, and, subsequently, Mr. Wilson, placed these muscular fibres in a part of the urethra where they did not exist. While Mr. Hancock willingly concedes to Kölliker the priority of noticing these fibres, yet he claims for himself the credit of describing their situation and arrangement, and their importance as bearing upon practical points. Mr. Hancock goes on to say]

I will now as succinctly as possible give you my own version of the subject: it is essentially the same as that which I announced in February last; but having since then bestowed considerable pains upon it, I have been enabled more completely to convince myself of the correctness of my views, and have therefore more confidence in submitting them to you.

The organic muscular fibres in the prostate gland, connected with the urethra, are continuous with those of the internal muscular coat of the bladder, whence they may be traced by careful examination, passing forwards through the prostate gland; these fibres, destined to invest the membranous and other portions of the urethra, appear to me to be entirely distinct from the organic muscular fibres found in large quantities throughout the gland; particularly around the sinus pocularis in the verumontanum or caput galliginis, where the principal excretory ducts of the gland, with the common ejaculatory ducts, open. Organic muscular fibres surround the various ducts which permeate the gland in all directions, and may, in the instance of the common ejaculatory ducts, be traced into the gland from the vas deferens, where they may readily be seen.

The same arrangement obtains around the proper excretory ducts of the gland, and is beautifully shown where calculi are present in any quantity or size, in which case the foreign body may be seen impacted in the duct or cell, with a circle of these organic fibres surrounding it.

The muscular fibres of the prostate are best seen in the prostate of a fœtus of between six and nine months, at which age the muscular fibres are very distinct, having large nuclei. In old age the muscular fibres, though readily traced, are not so distinct, owing to the phosphatic deposits and fatty degeneration which take place in the prostate gland at that period of life.

The organic muscular fibres found generally throughout the prostate gland, belong in a great measure, I believe, to the numerous vessels and ducts which ramify so freely through this body, as Mr. Guthrie has pointed out, and Mr. Quekett has proved the existence of muscular fibres in the coats of arteries; but these general fibres are, as I have before observed, distinct from those derived from the inner layer of the muscular coat of the bladder, and which form a layer surrounding the prostatic portion of the urethra, separated from it merely by elastic and non-elastic areolar tissue. (Kölliker says these fibres, for the most part, have no connection with the muscles of the bladder.) The outer layer of the muscular coat of the bladder, on the contrary, passes forwards on the outside of the prostate *gland;* and laterally and inferiorly joins the fibres derived from the inner coat in *front of the prostate* gland, to assist in forming the organic muscular covering of

the membranous portion of the urethra. Whilst, superiorly, or on the upper surface of the gland, these external longitudinal fibres are arranged in two or more bundles, which are attached, as Mr. Guthrie pointed out in the year 1830, to the pubes near its symphysis. From the front of the prostate the conjoined layer of organic fibres passes forwards to the bulb, investing the membranous portion of the urethra, covered by, but distinct from, the common muscles of the part, the latter being inorganic, voluntary, or striated; these being organic and nucleated. Arrived, however, at the bulb, these two layers again part company and extend forwards through the whole length of the spongy portion of the urethra, the internal layer running between the corpus spongiosum itself and the urethra, but separated from the latter by areolar tissue; the external lying on the outside of the corpus spongiosum, separating the proper spongy tissue from its fibrous investment. Upon reaching the anterior extremity of the urethra, these two layers again unite, and form a circular body or band of organic muscular fibres, constituting that peculiar structure usually denominated "the lips of the urethra," and which had previously been considered by Mr. Guthrie as surrounded by a peculiar dense structure, analogous to that which forms the edge of the eyelid, and which he believed was requisite to maintain the patency of the opening: so that not only have we the urethra supplied by a coat of organic or involuntary muscular fibre, but the spongy body itself lies between its two layers of involuntary muscle; an arrangement, doubtless, of very great importance in relation to the due performance of the functions of the part. And, as regards the urethra, this arrangement holds good wherever we find the spongy tissue, whether the quantity of that tissue be small or great; for, at the glans, which is formed not only by increased development, but also by a folding back, as it were, of the corpus spongiosum upon the corpora cavernosa, we have these muscular layers multiplied; whilst on the upper surface of the urethra, where there is merely a narrow portion of corpus spongiosum, the same arrangement holds good. Independent of these layers of organic muscular tissue, nucleated fibres may be found distributed occasionally throughout the spongy tissue, but I think they belong more properly to the arteries of the part.

When I commenced this inquiry, and when I submitted the results thereof to your notice, I was not aware that the subject had previously met with any attention. I had never seen nor heard of Kölliker's discoveries; I knew that the muscularity of the prostate gland had been hinted at; but I was not aware until I spoke to Mr. Quekett upon the matter, that, although he had not published, he had some years previously established the fact, but had not pursued the matter further. Therefore, as regards the prostate gland, the credit of priority is due to this gentleman.

Although Kölliker noticed muscular fibres in the prostate gland, over the membranous portion of the urethra and in the corpus spongiosum, he has not traced any connection between them, nor has he attempted to reduce them to anything like a systematic arrangement. He has not, for instance, traced the connection between the muscular fibres of the prostatic portion of the urethra and those of the bladder—a point of the greatest importance, as I shall hereafter prove to you, in the physiology of micturition. He does not trace the external layer of the muscular coat of the bladder over the outer surface of the prostate gland itself. He deprives the vesicles and ducts of the prostate gland of their muscular investment, whilst he almost denies a muscular membrane to the spongy portion of the urethra, observing "that it seems more natural to regard the whole corpus spongiosum as a highly developed muscular layer provided with peculiar blood-vessels."

Now, I repeat, that I claim the credit of having done what Kölliker has omitted to do—traced these muscular fibres continuously from the inner and outer layers of the muscular coat of the bladder to the distal orifice of the urethra, showing of what the lips of the urethra consist. I have shown that the vesicles and ducts of the prostate gland are surrounded by muscular fibre, those of the ejaculatory ducts being derived from the muscular coat of the vas deferens. I have shown that the spongy portion of the urethra has its muscular membrane or coat equally with the other regions of that canal, and I have also proved that we cannot correctly regard the whole corpus spongiosum as a highly developed muscular layer, but as consisting of two muscular layers, with an intermediate layer of erectile tissue. The distinction between these layers may readily be seen by the microscope, as well as

by injection; and I must confess I do not understand how we are to reconcile Kölliker's theory with the functions of the part. I do not understand how a muscle, however highly developed that muscle may be, can by any possibility perform the functions of the corpus spongiosum. The corpus spongiosum is not merely an urinary apparatus, but a genito-urinary organ; it has not merely to surround and influence the urethra, but to form part of, and that a most important part of the genital organ, and to qualify that organ for the due performance of the sexual functions; and to enable it to fulfil this double office, it requires its conditions of relaxation and erection. I shall hereafter have to notice the influence exerted by the muscular fibres of the urethra over micturition, which is ordinarily performed, or at all events most readily performed, whilst the corpus spongiosum is relaxed. I need scarcely allude to the difference in size between the corpus spongiosum in a state of relaxation and in the opposite condition of erection, when it is enlarged in all directions, not only in length and in depth, but in breadth also; and I would fain ask in what other instance in the body does this obtain?—in what other organ do we find, under any circumstances, healthy muscular fibre, whether organic or inorganic, increasing in bulk in all directions during its period of action? On the contrary, this theory of Kölliker's is opposed to one of the fundamental laws in physiology, regulating muscular contractility, that what a muscle during action gains in breadth it loses in length, and *vice versâ*.

We can however much more readily understand what I have demonstrated, that in the corpus spongiosum we have two layers of muscular fibre with intervening erectile tissue; we can more readily reconcile this fact with the functions of the part; and I believe that whilst the inner layer influences micturition, the two layers combined, when the state of erection is no longer required, serve to compress the erectile or spongy tissue between them, and squeeze the blood from the cells and blood-vessels, thus restoring the part to its ordinary and relaxed condition.

But whilst I am anxious to assert my own claims, I am equally desirous of acknowledging the obligation I am under to my friend Mr. Hogg, who has throughout pursued these inquiries with me, and rendered me the greatest aid in carrying out my investigations and confirming my views.

Spasmodic strictures may now be readily explained: the existence of this muscular layer continuous throughout the canal, offers the most simple, and at the same time satisfactory solution of our difficulties, and we have no longer occasion to attribute to sympathies what we can now prove to be muscular contractility. True spasmodic strictures have been ascribed to the spasmodic contraction of the acceleratores urinæ and Guthrie's muscles, whilst the phenomena met with in those situations to which these muscles did not extend, were attributed to sympathy, to sympathetic contraction, or sympathetic irritation, a species of pseudo-contraction; and we frequently found the sympathies as difficult of management as the actual and exciting diseases. We now find, however, that these contractions are not spurious or false, but actual *bonâ fide* muscular action of abnormal character, excited no doubt by morbid irritability of some portion of the canal. That these may exist, and probably do exist, coeval with spasmodic contraction of the acceleratores and Guthrie's muscles, I do not deny: but I believe, in opposition to what has been advanced by Sir B. Brodie, that a spasmodic contraction of these organic fibres may take place in the anterior part of the urethral canal, even within an inch of the orifice, and also that it may exist as a primary and independent affection, without a spasmodic stricture necessarily existing at the same time in the membranous portion. These spasmodic affections frequently accompany organic strictures; and I have at the present time a gentleman under my care who has had organic stricture of some twelve years' standing: it is a tough, cartilaginous stricture of, as nearly as I can judge by careful examination, about an inch in length, but the principal difficulty against which I have had to contend, has been a spasmodic contraction of extreme irritability about three-quarters of an inch from the orifice; this resists the instrument for some little time, but by gentle pressure it gradually yields, and I have then but little difficulty in getting through the original stricture, which feels as though the instrument was passing through tough unyielding structure. At first the pain produced by any attempt to pass the spasmodic contraction was so excessive that the patient roared with agony, and there was no

managing him; but by applying caustic once or twice this has been allayed, and we now get on very well.

In another patient the instrument is arrested about half an inch from the orifice, but after pressure for a short time the spasms give way, and the bougie then passes into the bladder, without my being able to detect any further obstruction; and this patient complains, when the instrument is in, of a grasping sensation at the point of obstruction, but at no other part of the urethra. The instrument is certainly held, and I have observed in this patient, when the sensation has been most severe, that the lips of the urethra have worked or wormed around the instrument, precisely similar to the vermiform undulations of the intestines, although of course in a minor degree. These cases, I submit, go far to throw doubt upon the dependent character of these contractions in the interior portion of the urethral canal, and tend to prove them as independent contractions of the muscular fibres of the part, sympathetic with, and probably influenced by, irritation of the urethra elsewhere, but not necessarily coeval with, or depending on spasmodic contraction of the inorganic muscles of the urethra, as is so insisted upon by some authors. Mr. Adams, for instance, in the article 'Urethra,' says the term spasmodic stricture can only, with strict propriety, be applied to a temporary contraction of the muscles investing the membranous portion, as no action of the acceleratores urinæ is equal to the complete closure of the spongy part. The common seat of the spasmodic stricture is therefore the membranous part; and he goes on to observe: "Although genuine spasm is confined to the membranous part, yet the remainder of the urethra is liable to temporary contraction from general irritation of the mucous surface. This condition probably depends on the irritability of those fibres, be they muscular or not, which enter so largely into the structure of the outer layer of the urethra."

I am certainly somewhat surprised at the doubt here implied, of the muscularity of the fibres surrounding the spongy portion of the urethra; but be that as it may, I differ from the doctrine which limits the seat of spasmodic stricture to the membranous portion of the urethra, and which confines the cause of that spasm to morbid action of the inorganic muscles of that part; and I also differ from what is here attempted to be inculcated—that the temporary contractions of the other parts of the urethra depend upon the general irritability of the mucous surface, which would imply that the impediments here met with to the passage of instruments are of a general, congestive, clinging character, rather than of the sharp, grasping, clutching kind, confined to certain limits, which we so frequently meet with. I have no hesitation in saying that I have met with as distinct a spasmodic contraction within an inch of the orifice of the urethra as I ever did at the membranous portion: indeed, it was a case of this kind which first led me to examine the minute structure of the part microscopically. It may be urged, that in the cases to which I have just alluded, by the application of caustic to the anterior stricture, the irritation of the whole urethra was allayed, accounting for the absence of difficulty in passing the instrument through the rest of the canal. But, allowing this to be the case, it is rather an argument in support of my views, that these spasmodic contractions may occur independently in any part of the canal, and need not necessarily be secondary to, or depending upon, a stricture, either organic or spasmodic, at the membranous portion; for analogy bears me out in the assertion, that in no other instance do we find, that by attacking sympathies or effects, we can conquer or remove the exciting cause.—*Lancet, Feb.* 21, 1852, *p.* 188.

114.—STRICTURE OF THE URETHRA AND PERINEAL SECTION.

By William Fergusson, Esq., F. R. S.

[In a clinical lecture, Mr. Fergusson makes the following observations on stricture of the urethra and fistula in perineo—more than usually interesting at present on account of the controversy respecting the treatment of these affections:]

Suppose the surgeon meets with a very bad case of irritable stricture, and which does not yield to instruments, can he do anything more? It might perhaps be set down as a maxim in surgery, than he can do no more; but maxims of surgery

are not immutable, and in this art there is still much room for improvement. Now I think it must be admitted that we are indebted to Mr. Syme for proposing division of a stricture under these circumstances. Division of a stricture by external incision, however, is by no means a modern practice; it is as old as the time of Wiseman, and it has been followed since, from date to date. The surgeon has been induced to operate in consequence of his inability to pass a catheter, in infiltration of urine, where there have been bad fistulæ, and where life has been placed in jeopardy. Still the operation has been by no means of frequent occurrence; but this practice has been put in force with the very best results in such cases. I myself have had numerous opportunities of performing what we may now term, the old operation of perinæal section, and with very great success.

But the practice which has been recommended by Mr. Syme is different from that I have just been speaking of. The proposal of that surgeon is, to divide the urethra externally in certain cases of irritable and contractile stricture which resist the ordinary practice by the bougie; and he makes it an essential feature in his operation, that a small sound should be previously introduced through the stricture, and that the division should be made upon this.

Now, my own opinion is, that in certain cases it is an admirable operation, and gives relief; but then I think it is an operation which ought not to be applied without great consideration, and not until all other modes of treatment have failed; for although some cases have turned out well, much mischief has been done in others. I myself have seen death result from it, and also danger of the worst possible description, and am so impressed with this, that I must beg of you to be very cautious before you resort to this so-called perineal section. Its true value remains yet to be proved; the time is not yet come for us to say much positively of it. We have not yet had sufficient experience to say that it will prove generally serviceable. One of the most satisfactory cases I have had was in a gentleman who suffered terribly from stricture and its consequences. He was at first under the care of the late lamented Mr. Liston, who treated him by bougies with relief. At that surgeon's death, he came under my care. His chief symptom then was a succession of aguish fits, which were most violent, and in fact the patient himself thought that he had regular ague. However, I found that he had a very troublesome stricture. There was excessive irritability when an attempt was made to pass instruments, and it was followed by a severe attack of shivering. No benefit was derived by the attempts at dilatation, although I could pass a No. 3 or No. 4 catheter, and it appeared to me that the patient's constitutional suffering was entirely dependent on the state of his urethra. With a view of effecting a permanent cure, I proposed to him that I should cut his stricture and relieve his ague at the same time. The operation was done; the patient had no bad symptom after it, and all his previous distress went away. This is now three years ago; within the last twelvemonth, however, some of his former bad symptoms have returned, and in consequence of his having neglected to pass bougies; and he has lately been to town to have instruments passed, as the urethra had again contracted. Now, here you have an instance where the operation was attended with great benefit; but still it shows, that after all, there is a tendency in the disease to return, and that the cure is not a permanent one.

I will now just draw your attention to the case of the man Abraham, on whom I performed the same operation. This case may make us doubt as to whether it was the best thing that could be done for him. I could pass an instrument here, but, nevertheless, the fistula in the perinæum would not heal, and I therefore performed the operation according to Mr. Syme's plan; but those of you who have watched the progress of the case will see that he is not much better. There is now —more than a month after the operation—a great deal of irritation about the bladder; he passes his water frequently, and it contains a great deal of mucus and pus; the wound has not closed up; in fact, instead of his being well a week or two after the operation, he is now perhaps not much better than before. However, we must not judge hastily of this operation from this case alone: we may probably have some further opportunities of testing the value of this proceeding; and until we have had some further experience of it, it will be well if we suspend our judgment regarding it.

In Barton's case first spoken of, there was impervious stricture; in fact, I could

not pass any instrument after many attempts, and you remember he was suffering from a most severe symptom, namely, a constant dribbling of urine. The bladder was distended; so much so, in fact, that it could contain no more water, and it kept dribbling away. This man himself was quite unaware that his bladder was in this state. Under these circumstances, and finding that I could not pass any instrument into the bladder, it seemed to me that it would be well were I to try incision. Hitherto the patient had been treated in the ordinary manner,—that is to say, I had used the catheter on my visits to the hospital while he was in bed, but I now determined to have him brought into the theatre to place him fully under the influence of chloroform, and then make a last attempt to pass an instrument; if this failed, to perform the perineal section. You saw, that after two or three patient attempts, I was enabled to break down the firm stricture, and pass the catheter into the bladder. I did not use a small instrument, because it is much more dangerous than a large one under such circumstances. The catheter was tied into the patient's bladder, and allowed to remain. In the course of ten days I could pass a No. 10 instrument.

It is worthy of remark, that in this instance the patient bore the presence of the catheter uncommonly well. A great difficulty is often met with in the circumstance of patients not being able to bear an instrument in the urethra for any length of time. It produces great distress, and brings on general irritative fever, but there was nothing of the sort in Barton's case, and hence we were able to open up the passage so rapidly. Now, as regards the practice adopted in this instance, there is no novelty in it; but I do not think it is often resorted to. I must confess to you, that the instrument was forced through the urethra, or it may have been pushed through the diseased tissues as near the urethra as possible; but the surgeon is not to be blamed for adopting this mode of practice under circumstances of a like nature. He must, however, be excessively cautious in what he is doing. After a stricture has been forced in this way, it is an admirable practice to keep an instrument in the urethra for some days if it can be borne, for the pressure of the catheter keeps it open and causes absorption from the indurated tissue, just as pressure by means of a firm bandage effects the dispersion of the induration around an old ulcer.

In a case of this kind the instruments of Mr. Thomas Wakley might be tried, and probably with success, if mere rapid dilatation would suffice; but, unfortunately, we cannot always depend upon it, for it will happen, that, after having rapidly dilated a stricture, there may be great irritation afterwards, and ere long matters may be as bad as before.—*Med. Times and Gazette, March* 13, 1852, *p.* 258.

115.—*External Division of Stricture of the Urethra.*—Mr. Coulson related the following cases of cure of impermeable stricture by Syme's operation:—Wm. J——, aged forty-four, a sailor, was admitted into St. Mary's Hospital, Oct. 13, on account of great difficulty in passing his urine. Eight years ago, he had stricture, but did not suffer much from it till the beginning of 1849, when he met with an accident, at which time he was seized with retention of urine, and has suffered from it occasionally ever since. At the time of his admission, he passed his urine sometimes in a very fine stream, at other times drop by drop. On passing an instrument into the urethra, a firm obstruction was met with five inches down, through which no instrument could be passed; just anterior to this, there was a false passage. There was no induration beyond the stricture, nor any perinæal fistula. The man had been in one of the largest metropolitan hospitals for some time previously, but no passage could be effected through the stricture. Under these circumstances, Mr. Coulson thought the case a favourable one for operation, and on the 15th of October, he divided the stricture through the perinæum. A grooved staff was introduced down to the stricture, and the urethra opened just above it, immediately below the scrotum, for an inch and a half; the stricture was then divided, and the staff readily entered the bladder. This was withdrawn, and a No. 8 silver catheter introduced, and retained in the bladder forty-eight hours. There was some little difficulty in the operation, for no instrument being passed through the stricture, there was no guide to the urethra beyond the contraction. Another cause of difficulty was the existence of the false passage, for the staff, on being

pressed against the stricture, had a constant tendency to go in the wrong direction. Mr. Coulson said that he might probably have passed a No. 1 grooved staff through the stricture; but he made fewer attempts than he otherwise would, in consequence of the false passage, and his having frequently opened the urethra easily, without the assistance of a staff. There was a good deal of bleeding after the operation, but it stopped in the evening. A No. 8 silver catheter was introduced every third day, whilst he remained in the hospital, and retained half an hour at each introduction. The patient had not had a single bad symptom; the wound healed readily, and the patient was discharged at the end of five weeks. Mr. Coulson said that he had seen the patient two days ago, and the largest-sized catheter could be easily passed into the bladder.—The second case was that of a patient, aged fifty-four, who had laboured under stricture nearly thirty years. It had been divided by the lanceted stilette, and periodically dilated; but during the last two years it had become so much contracted, as not to admit a No. 1 bougie. The water came away only drop by drop; and at times retention occurred, which was always relieved by the application of the potassa fusa. Mr. Syme's smallest grooved staff was passed, and an incision an inch and a half in length was made, commencing from close to the scrotum, and carried down towards the anus. The groove of the staff was at once reached, and the stricture divided. A No. 8 silver catheter was introduced, and a considerable quantity of fœtid, alkaline urine discharged. There was a little bleeding in the night, which was easily stopped by pressure. The catheter was retained in the urethra forty-eight hours, and introduced every third day for a fortnight, at the end of which time this patient was able to leave the house, and resume his duties in a large newspaper establishment. At the present time, the largest catheter (No. 10) which the orifice of the urethra will admit, passes readily into the bladder. Mr. Coulson stated that he had operated on other cases, but he would bring them before the Society on another occasion. Mr. Syme's direction for the performance of the operation, is, that a grooved director, slightly curved, should be passed through the stricture; and the surgeon, sitting, or kneeling on one knee, should make an incision in the middle line of the perinæum or penis, wherever the stricture is situated. The incision should be about an inch or an inch and a half in length, and extend through the integuments, together with the subjacent textures exterior to the urethra. The operator is then to feel for the stricture, and introduce the knife, or small straight bistoury, behind or on the bladder side of the urethra, dividing, from behind forwards, the whole of the contracted portion. Mr. Coulson said that many severe cases of stricture will yield to persevering efforts at dilatation, or the use of potassa fusa, or the ingenious and valuable instruments of Mr. Thomas Wakley; but every now and then cases will occur which resist all these means. Some years ago he had published several cases of stricture which he had cured by the lanceted stilette, after the failure of attempts at dilatation and the use of caustic, but there is an uncertainty in the use of this instrument which does not exist in the operation recommended by Mr. Syme. He said that, among the many and great obligations under which Mr. Syme had placed the profession to him, not the least was that of having recommended an operation so easy of performance, and so effectual for the class of cases to which those he had mentioned this evening belonged.—*Lancet*, *Dec.* 27, 1851, *p.* 610.

116.—*Mr. Syme's Operation for Stricture.*—[The editor of the 'Medical Times' having given Mr. Syme the credit of reviving an old operation only, referred him to an account of the incision extérieure or boutonnière in Malgaigne's 'Manuel de Médicine Opérative,' p. 651. The following is the passage alluded to.]

"A grooved staff or catheter is carried down to the obstruction and held there by an assistant. The surgeon then makes a long incision over the lower surface of the urethra, and cuts upon the conducting instrument, which, at the same time, is slightly withdrawn. He next searches for the continuation of the canal at the bottom of the wound, the patient at the same time trying to make water, and endeavours to introduce, where he sees it flow, a director or probe to guide his incision *through* the remaining part of the stricture. A catheter is then placed in the *bladder, and allowed* to remain until the wound is healed."

[Mr. Syme replied that as this operation only applies to impermeable strictures, of which he denies the existence, it can have no reference to his operation. In answer to this objection, the editor refers him to the following passage from the Surgical Works of P. J. Desault, edited by Bichat—1803:]

"It is difficult to form an exact idea of the operation of '*la boutonnière*' from the writings of authors, either ancient or modern. It was practised in so many different ways, and the operative proceedings offered so much contrariety and so little resemblance, that it is impossible to regard the subject in any general point of view." . . . .

"The same mode of proceeding is not always followed in performing '*la boutonnière*' upon the canal of the urethra. *When a catheter can be introduced into the bladder, this instrument is used that the incision into the canal may be made upon its groove, and that it may conduct a gorget, which will serve to facilitate the introduction of the canula, which is to remain in the bladder.* Here the operation presents no more difficulties, nor any more danger, than the incision for '*la talle au grand appareil*,'—'lithotomy with the great apparatus;' but then it offers no advantages in the treatment of retention of urine, for, *since a catheter can be introduced, it is also possible to pass a sound, which would serve for the evacuation of the urine, and to re-establish by its presence, the free passage of the canal.* When we cannot succeed in introducing the catheter, the operation becomes much more embarrassing," &c.—'Œuvres Chirurgicales,' de P. J. Desault. A Paris, chez Méguignon l'Aîné, Librarie, Rue de l'Ecole de Médecine. An. xi. 1803. Tome III., p. 330.—*Med. Times and Gazette, Jan.* 17, 1852, *p.* 72.

117.—ON SPERMATORRHŒA.

By John L. Milton, Esq.

Mr. Milton offers the following observations on the treatment of this disease. He says:]

It consists,—

1. *Of Quinine in Solution*, in the following form:—

R. Quin. disulph. gr. vi.; acid sulph. dil. ℥j.; tinct. cardam. co. ℥iii; aq. cinnam. ℥ vss. M. Sumat cochl. duo ampl. bis die.

Used in this way, one grain seems to have much more effect than larger doses with less acid.

2. *Of Local Baths of Cold Salt-Water.*—As sea-water is often difficult to obtain, and the class of patients most subject to this disorder are extremely averse to a measure likely to induce suspicion that they are labouring under any disease of the generative organs, I generally direct the patient to buy a pound of common salt, break a piece off as large as a walnut, and dissolve it in half a basin of water. The scrotum and perinæum are then bathed with this by the aid of a sponge for five minutes every morning, and the water thrown away, so that nothing remains to excite any suspicion; those patients who are under no restraint may use a hip bath of cold solution of salt with the greatest advantage.

3. *Some Gymnastic Exercise every Day.*—The application of this remedy must naturally be modified by the patient's position in life; but even those most restricted can take a walk early in the morning and last thing at night. When this trenches on the hours of sleep it may be regarded rather as an advantage than otherwise; the less sleep the patient has the sounder it will be; the earlier he rises the better, the erections being generally most forcible and recurring most regularly in the morning.

When the weather does not admit of out-of-door exercise, I advise reading for a fixed time, as an hour or so every night; and if the patient be restless and unsettled, reading aloud, even when he is obliged to walk to and fro to accomplish his task, will often soothe down this excitement and dispose him to sleep.

4. *Of Checking the Erections.*—It will often be remarked, that the patient has erections two or three nights successively. When these awaken him, I find it best to treat the case like one of chordee, and direct him to rise and take a teaspoonful of spirit of camphor in water. This will generally allay the priapism, and prevent

its recurrence. On those nights when he expects the emissions, a dose may be taken last thing at night.

The bowels should be kept loose; and, for this purpose, five grains of blue pill may be taken occasionally, in conjunction with rhubarb. When this does not act, the sulphate of magnesia may be added to the mixture.

5. If these measures do not suffice to cure the disease, I would advise blistering. If applied on the perinæum, it acts most efficiently; but, in some instances, I have seen this followed by a troublesome crop of boils; this I have never seen from a blister on the penis, where it can be applied and dressed much more easily, and where it occasions much less soreness and difficulty in walking. If the patient objects to this, it may be laid on the groin.

Those cases in which I tried steel failed. In place of acting beneficially, it seemed to heat and over-stimulate the patient, and even to dispose more to erections. In that shattered state of the frame in which the semen passes away involuntarily, and almost without an erection, it may be useful, but I am inclined to rely more on the measures I have laid down.

Some surgeons, considering this disorder, in many instances, as merely an effort of nature to throw off an accumulated secretion, recommend connexion. I would neither recommend nor forbid it, unless I found it acting injuriously. In some cases the patient takes it for granted, that if this be THE remedy, medicines, gymnastics, &c., can do him no good, and shaking off all restraint, gives way to the worst excesses. The plan, too, is not free from danger to the surgeon's reputation. Only very recently, a patient placed himself under my care for seminal emissions. He had suffered under them for a long time, and had consulted a surgeon, who advised him to have connexion. The result was a gonorrhœa, which took two months to cure; and this mistake unsettled all his former confidence in his medical adviser.

When self-pollution or excessive connexion is indulged in, I only know of one remedy, and that is, the employment of some irritative ointment to the penis, such as that of bichloride of mercury, ℥ss. to ʒj., deut. iodide of mercury, ℈j. to ʒj.; or of the ung. ant. pot.-tart. I have found it most efficacious when applied only once a week, so as to keep the penis gently sore, as otherwise he may grow alarmed by the severe blistering and pain which the ointment occasions when used too freely.

Here it is useless to reason with the patient; he will even with the most sincere desire to give it up, continue his baneful practices, and those who have command enough over themselves to abstain during the day, often resort to this habit when asleep. The only way to break him of it, is to make the penis so sore, that he is at once awakened by the smarting so soon as he commences any attempts at friction. When once the habit is fairly broken off, he rarely recurs to it.

The despondency of spirits, the loss of appetite, flatulence, weakness, pain in the back, &c., under which many of these patients labour, are generally removed by adopting a plain diet, as weak coffee, toast, and bacon, for breakfast, an early dinner, consisting of a chop or two and bread, strictly excluding all porter, vegetables, cheese, pickles, or pastry; as little tea as possible; and in the evening, instead of supper, a basin of tapioca, ground barley, or arrow-root, with a biscuit.

A persevering use for a few weeks of this treatment will, I think, effect a cure even in the most inveterate cases.—*Med. Times and Gazette, March* 6, 1852, *p.* 241.

118.—ON NITRATE OF SILVER IN SPERMATORRHŒA,

AND A NEW INSTRUMENT FOR APPLYING IT.

By Dr. HENRY THOMPSON, formerly House-Surgeon to University College Hospital.

[Increased experience has only the more proved the great value of the application of the nitrate of silver to the prostatic portion of the urethra, as first suggested by M. Lallemand in certain cases of involuntary seminal discharges.]

Among the many modes of treatment applicable to a certain, and by far the largest, class of these cases of spermatorrhœa, as they are usually termed, that of passing down to the prostatic portion a piece of nitrate of silver, and exposing it *for a moment, so* as rapidly to cauterize the surface on which the prostatic and

seminal ducts open, has been extensively practised, and found useful. This is, notwithstanding, by many still regarded, as it was at first almost universally, to be a somewhat severe mode of treating the delicate mucous membrane of a narrow and most important canal; the sphere of operation being far removed from the guiding hand, and completely so from the direction of the eye. The use of so potent an agent, with such little powers of control as to the manner, and even as to the exact extent and locality, of its application, was, *a priori*, looked upon as "heroic," or at all events only indicated in cases of great need. This feeling has naturally led to a more limited use of M. Lallemand's apparatus than is perhaps desirable, and certainly prevented the *principle* from being brought into action in many cases in which its application would have proved most serviceable, and in which nothing else at present known can so efficiently replace it. Indeed, the want of a modification of this valuable remedy might almost be inferred from the words of Lallemand himself, who most candidly confesses and enumerates the dangers which he has encountered as the results of the operation. But to myself this has been most obviously manifested in practice, from having observed, that although the application of the crude caustic has ultimately effected the beneficial purpose intended, it has often done so at a far greater expenditure of action, attended by pain and inflammation, than was necessary for the cure, or of course desirable for the patient. It involves the act of summoning to our aid a greater power than is necessary in order to remove the morbid condition, which surplus of power is thus expended upon the organ itself, and greatly, as will appear, to the disadvantage and injury of its delicate and complex structure.

[Unless, however, there is absolute rest on the part of the patient, however delicately and carefully the application may have been made, this remedy is very liable to be followed by serious consequences, such as retention of urine, hemorrhage, and inflammation.]

But there are many patients who would benefit greatly by the treatment, with whom the necessary rest is almost an impossible condition; young men whose avocations will not permit them, without most inconvenient and serious consequences, to comply with M. Lallemand's injunction. This will be at once obvious to all, and the consequent necessity for a mode of treatment, if possible, adapted to their circumstances; and for such, the modification to be proposed is suggested to be especially useful, as being perfectly compatible with a moderate amount of exercise. And not only so, but there is a degree of inquisitive attention drawn to the patient's condition on the part of friends when operated upon at his own house, and confined there for days, which it may be difficult, but in most cases extremely desirable, to parry, and which sometimes renders the use of the "porte caustique" a very serious undertaking in relation to the subsequent necessities of the patient. Now in the treatment of these cases, it naturally occurred to me, that as in the affections of all other mucous membranes requiring the application of the nitrate of silver, we accomplish it often more advantageously in the condition of solution, made of various strengths—*e. g.*, from one grain of the salt to one ounce of water up to two drachms to the ounce,—we might also apportion, as experience should indicate, our remedy, in the matter of strength, to the management of these prostatic affections also, and not invariably use the crude anhydrous salt as heretofore. The anomaly appears more manifest when we consider that in this particular locality its use is less manageable than in any other, and its excess more disastrous in its results. And I must add, that experience has most completely confirmed the truth of this view. There are very few cases indeed in which, as far as I am able to judge, the strongest solution above-named will not answer all purposes, and accomplish the desired result quite as efficiently, and certainly with more ease and less danger to the patient: so that the cure shall be at all events accomplished more in conformity with the old maxim, "Cito, tuto, et jucunde."

Next comes the mode of its application. This problem I have endeavoured to solve for myself by constructing an instrument for the purpose, at first made to meet a special case; and now that some experience has rendered it more perfect, I beg leave to submit it to the profession in its present form as, I believe, perfectly safe, manageable, and easy of application. It was made from my design by Mr. Coxeter, of Grafton-street, and I can only say that he has embodied the idea with

great accuracy and skill. It consists of a curved canula,* having nearly the form of an ordinary catheter, in size about No. 9 or 10; within this, there is a strong stilette, at one end of which is a well-fitted piston adapted to the bore of the instrument, while the other protrudes from the upper part of the canula, and is shaped into a handle. Immediately below the piston is a piece of sponge about an inch long, cylindrical, and having the same diameter, immovably riveted between two little plates of metal, into which the stilette here divides, and beyond this there is an oval metal extremity, which is made to fit the end of the canula so closely as to form with it, when shut, a conical sound for the purpose of introduction. Its firmness there is secured by means of a screw at the opposite end, so contrived as to clamp the rod and render it immovable in the canula, when made to act by turning it in the usual manner. Thus it may be used as a common sound, and carried down to the spot at which the patient begins to feel severe pain, usually at the commencement of the prostatic portion. The slide, for the purpose of marking this spot, is pushed down to the glans penis, the organ being moderately elongated, and the instrument is withdrawn for the purpose of receiving its charge of solution. The mode of charging is as follows:—Make the sponge to protrude into a bottle containing solution of the strength required, by pushing down the handle, and withdrawing it again, while still immersed in the liquid. This process is to be repeated once or twice if the sponge be dry. By means of the piston, two or three minims are carried up into the canula, in addition to that which enters the substance of the sponge, and then the oval extremity closely shuts all up within. This being secured tightly by means of the screw aforesaid, the instrument is wiped, oiled, and warmed for introduction, not a drop being able to escape until the operator, by pressing on the handle, causes the sponge to protrude in any part of the urethra he pleases, and apply the solution to it at once on all sides; then, while still in situ, by withdrawing the sponge into the canula, two or three drops more fall upon the floor of the canal. The instrument is now removed altogether. There is no rotation of a metallic "cuvette," which is the very painful part of the proceeding with the original "porte caustique." And I am prepared to say that the cauterization is not only equal to that accomplished by the old mode, but to go farther, and assert that it is better adapted, from the form and structure of the prostatic portion of the urethra, to accomplish the end in view, since, while it applies the solution to every part, the floor receives the largest share; and actual experiments on the dead body, followed by dissection, in which I have used for comparison M. Lallemand's instrument and that here proposed, show, beyond dispute, the more entire and equable distribution of the caustic by the latter instrument.

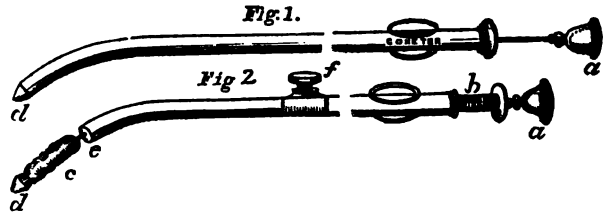


Fig. 1. The instrument closed. Fig. 2. The instrument open.
a. Stilette and handle, to the opposite end of which the piston and sponge are attached. *b.* Screw which clamps the stilette when closed, as in Fig. 1. *c.* The cylinder of sponge which contains the solution. *d.* Oval silver bulb which closes the canula. *e.* End of the piston just seen, the sponge being protruded to its utmost extent. *f.* Movable slide.

I must notice, further, a strong objection to the former apparatus, which I have not only observed, but heard stated by others who constantly use it, and which is apparent enough on a cursory examination, namely, that in rotating the "cuvette"

* Mr. Coxeter makes this canula of silver, but *without a seam* in it—an improvement which obviates the necessity for using platinum in the construction of the parts which come into contact with the caustic itself, as in the present "porte caustique." When tubes are made with a seam in the ordinary manner galvanic action is set up, through the solder necessarily used, when moisture is present, and rapid wear ensues. Hence the use of platinum, the necessity for which and the consequent cost are thus superseded in this instrument.

in order to expose the whole circumference of the urethra to the caustic, which is accomplished by turning the handle with the finger and thumb, its distal end describes a circle of three-eighths of an inch, or more, in diameter, and does not merely turn on its own axis, which is the movement desired; and this occurs in the most tender part of the passage, where in most cases even the mere presence of a sound is extremely painful. This will be at once understood by a reference to the instrument, and observing the motion of the cuvette while a rotatory action is applied to the handle.

I must now bring this paper to a close, as it has already become much more lengthy than it was at first intended to be, and reserve some remarks on other matters connected with the subject for a future time. One more only shall be just mentioned here. Every one meets occasionally with a case of obstinate gleet, which seems to withstand the usual routine of treatment, or indeed almost any other. Such have been at times described as depending on sub-acute inflammation, or some other obscure affection of the prostate gland. Without pursuing the question of their true pathology on the present occasion, I would suggest the application of the nitrate to the prostatic portion of the urethra—a solution, say, of one or two scruples to the ounce. I have found it successful as a last resort. It remains only now to add that Mr. Coxeter will exhibit the instrument to any who may deem it worthy of inspection; and I shall be most happy myself to show and illustrate its mode of action to any of my professional brethren who may feel sufficiently interested in it to call upon me at any time, as well as to receive any suggestion which may tend to the more perfect accomplishment of the object it is designed to attain.—*Lancet, Jan.* 24, 1852, *p.* 89.

[Mr. Henry Smith, of Bedford-square, in the 'Medical Times and Gazette,' of Feb. 14, 1852, lays claim to the invention of a very similar instrument to the one described above, which was made by Matthews, the solution being applied by means of sponge, fastened to a stilette, and passing through a canula.]

119.—*Treatment of Varicocele by Gutta Percha dissolved in Chloroform.* By Dr. H. G. Carey.—After having used gutta percha considerably for other purposes, a knowledge of its properties forcibly suggested it in solution, as admirably fitted to fulfil the desired objects sought in the treatment of varicocele. In order to apply it, the patient is placed upon his back, and by means of cold, the scrotum is corrugated until it is drawn firmly over the root of the penis, compressing the testes firmly in the upper portion of the inguinal pouches; then, by means of a camel's hair pencil, after the hair has been removed, apply the solution freely over the site of the scrotum, allowing it to extend on all sides some distance by a thin attachment; but over the scrotum proper lay on a succession of coats, until a thickness of a line uniform throughout is obtained, which will be sufficiently strong to form an artificial pouch of the nature and character desired. This thickness will be so yielding and pliable as not to afford the wearer any considerable inconvenience. Soon after the solution is applied to this sensitive part, the patient will complain bitterly of the burning sensation experienced, depending upon the presence of the chloroform; but this temporary inconvenience will soon pass off. The constitutional indications, if there be any, must not, of course, be neglected.—*Monthly Journal of Med. Science, March*, 1852, *p.* 261.

120.—ON VASCULAR TUMOUR OF THE URETHRA.

By H. B. Norman, Esq., Surgeon to the St. Marylebone General Dispensary.

[Mr. Norman invites attention to this subject rather on account of its relation to the condition of the female urethra than its possessing much intrinsic interest. Mr. Norman regards the fungi, which Mr. Broomfield speaks of in his 'Chirurgical Observations,' as situated upon both the male and female urethra, as the ordinary vascular tumour of the meatus. The following cases are recited by Mr. Norman as evidences of this fact:]

Case I. (Obs. 10), "is that of an old officer, who had for twenty years suffered from a polypous excrescence which came out of the urethra, near half an inch long, and which was considered as the offspring of a gonorrhœa which he had contracted in 1711. The vegetation was red, fibrous, softish, and almost filled up the orifice of the urethra. It rendered the *egress of the urine somewhat painful, and the stream twisted.* After all known methods had been tried without success, he went from Gibraltar to Montpelier, to place himself under the care of Mr. Fitzgerald, Professor of Physick to the Faculty there. The Doctor undertook his cure *conditionally*, promising to depurate his blood from any virus with which it must have been infected, but not to cure the excrescence. After a course of physick, which did not affect the excrescence, the Doctor advised him to cut it off, as he had done for *twenty years* before. The excrescence stuck by him fifteen years more, he having recourse to the same operation. At last it dried up of itself and never reappeared; but the urine did not flow freely enough, and in 1756 he applied to Mr. Arnaud, who discovered a very hard carnosity in the fossa navicularis, which he treated successfully by the medicated bougie."

Case II. "Mr. Key, in the year 1745, whilst in Flanders, was consulted by a gentleman of that city, who for six years had had an obstinate gleet; his urine involuntarily slipt away, so that he was obliged to wear cloths continually in his breeches to keep his linen dry; the stream of his urine was forked, and sometimes divided into many streams. On opening the orifice of the urethra (which had been lacerated by an accident some years before down to the frænum), Mr. Key discovered 'a bit of flesh,' and as it presented, he brought it forward with his forceps, and snipped it off with his scissors. By the use of medicated bougies, the gleet was cured in twenty-two days, and in the same period, several obstructions of the urethra, and three remarkable fleshy warts in the orifice, also yielded by suppuration."

Case III. Obs. 12 of the same work, is a case also related by Mr. Key, in which "incontinence of urine had existed eight years, occasioned by a carnosity situate near the bulb of the urethra, and accompanied by a running of purulent matter. Mr. Key undertook the cure by medicated bougies, which, after three weeks' suppuration, effected the expulsion 'of a fleshy substance as big a pea.' There was afterwards no more obstruction to the passage of the instrument."

Case IV. occurred recently at the University College Hospital, in the practice of my friend Mr. Erichsen, who kindly furnished me with the following account. "Robert Moggridge, 21 years old, presented himself to me at the hospital, for stricture. On examining the urethra, I found a bright red, and very vascular growth, situated within the urethral orifice. It was nodulated, raspberry-like, and bled on being touched. Its attachment was not pediculated, or but very slightly so, and the growth, which was about the size of a small cherry-stone, lay entirely within the urethra. I removed it with a pair of scissors; it sprouted again; was again removed, and the surface from which it grew was touched with nitrate of silver, after which it did not reappear. It was the sole cause of an obstruction to the passage of the urine, there being no stricture."

Case V. Finally, my friend Dr. Quain has informed me, that he was requested some time since, by a man under his care at the Brompton Hospital for Consumption, to examine his urethra. At its orifice, he found it presenting "a villous, spongy state, accompanied by some mucous discharge." This appearance struck him as being so peculiar, that it remained impressed upon his memory, although he had no recollection of the history or other symptoms of the disease, at the time he related the circumstance to me.

The characteristic features of the vascular tumour or excrescence of the male urethra, as described in the preceding narratives of cases, will be seen to bear a close resemblance to the varied forms of the disease in the female. In the case mentioned by Dr. Quain, we have simply "a red, villous condition of the orifice"; in that of Mr. Erichsen, "a distinct, nodulated, raspberry-like, *non-pediculated* excrescence, bleeding when touched"; in Arnaud's first case, "a polypous excrescence, hanging half an inch out of the urethra, red, fibrous, and softish"; in his second case, seen by Mr. Key, "a bit of flesh in the orifice"; and in his third, "a fleshy substance as big as a pea." Who that has seen much of the disease in the *other sex will fail* to recognise all these varieties?

The effects likewise are very similar: "a mucous discharge," "difficulty of making water," with "pain," "twisting and dividing of the stream," and "incontinence of urine;" being those named. The sensibility of the growths does not seem to have been in any instance very great.

The patients, it appears, were also of different ages, and the causes obscure or altogether unknown. In two instances, the growths showed a great tendency to grow again after excision, one was very obstinate, and there appears to have been no effort made to counteract the tendency, which at length indeed ceased of itself; in the other it was readily overcome by the application of caustic. In no one instance is there evidence of anything like a malignant nature having belonged to the growths; which, in all probability, had they been carefully examined, would have been found to have possessed the same anatomical characters as exist in the excrescences of the female.

In the treatment, the same means are, it may be presumed, equally applicable and equally serviceable in either sex; and, in the present state of our knowledge, when we have no facts to guide us as to the constitutional origin of the malady, we must be content if our therapeutics in these cases are not in advance of our pathology. Promises "to depurate the blood" by constitutional treatment, leaving the local disease to itself, would not now do either for physician or surgeon.—*London Journal of Medicine, March*, 1852, *p.* 236.

121.—*Prurigo of the Genital Organs.*—This affection (which often assumes the form of lichen or eczema) is well known to be very painful, distressing, and difficult to remove. M. Tournié has lately proposed calomel ointment, and a powder of camphor and starch, which topical applications he has used with much success. When the parts (genital organs, anal region, or axilla) are covered with scabs, tepid baths, and emollient applications are to be used first. When the indurated particles are removed, the affected spot is to be rubbed twice a day with the calomel ointment (one to two drachms of the calomel to one ounce of axunge), and after each application dredged with the powder (four parts of starch to one of finely powdered camphor). We mention M. Tournié's treatment as this kind of prurigo so often baffles the remedies usually employed.—*Lancet, Dec.* 13, 1851, *p.* 556.

122.—*Treatment of Hydrocele.* By Bransby B. Cooper, Esq., F.R.S., &c.—Many instances are recorded of patients having experienced a spontaneous cure of hydrocele, even without any apparent cause for the absorption of the fluid,—in young children this is not by any means uncommon. I lately had a case in the hospital of a boy eight years old, with a hydrocele on the right side. I performed the operation of acupuncturation, and the fluid entirely disappeared; about a fortnight after, it had, however, re-accumulated. An evaporating lotion, containing muriate of ammonia, was applied to the scrotum, but, as it produced not the least good effect, it was discontinued. The boy remained, however, in the hospital for other disease, and, when about fifteen days had elapsed after leaving off the use of any remedy for his hydrocele, the fluid began to be absorbed, and soon disappeared entirely. In children I never recommend tapping, but try first the effect of general treatment, applying merely a lotion to the part; the bowels should be opened freely, and the following lotion applied constantly to the scrotum:—

℞. Ammon. muriat. 3i.; sp. vini rectif., liq. ammon. acet., aa. ℥ij.; aquæ ℥iv. M. Ft. lotio.

Should such treatment not prove successful, I try acupuncturation, making two or three small punctures into the tunica vaginalis; and squeezing the fluid into the cellular tissue, so as to produce a kind of œdematous condition of the scrotum. I have seldom known this plan of treatment fail in children, but in adults it is only palliative. About three months since Mr. E. brought me his two sons; the elder about eight, the younger four years of age, both the subjects of hydrocele; the elder having also a hernia, for which he had worn a truss. In neither of the children could the fluid be pressed back into the abdomen. I performed acupunc-

turation in both cases; the younger boy was quite cured, but the hydrocele returned in the elder. I therefore punctured the tunic with the small trocar and canula used in tapping the chest in empyema, and drew off about seven drachms of fluid. Since this period there has been no return of the disease, but the hernia has had a tendency to return; the boy has, therefore, been obliged to resume the use of his truss. It is remarkable, in this case, that the two young brothers should have been thus simultaneously afflicted; but I have seen so many instances of this, that I have no doubt that there must be something like an hereditary tendency to hydrocele.

In simple hydrocele in the adult—in such a case for instance as that which I read at the commencement of this lecture—I should always first draw off the fluid, and not employ an injection; for I have found, in many cases, that mere tapping will produce a permanent cure. Should the disease return, I then use an injection of the compound tincture of iodine, one part of the tincture being mixed with three of water. I throw two drachms of this injection into the tunic, allowing it to remain. I have found it almost invariably successful, not having failed more than once in upwards of forty cases; and, in that case, I believe the fluid escaped through the canula. I therefore never employ any other means of treatment, and consider that it ought completely to supersede every other kind of injection, as well as the use of setons, caustic, and indeed all the older plans. It is possible, however, that even this injunction may fail. In that case, I should resort to a treatment that I formerly found to succeed after injections of port wine, &c., had failed. This treatment consisted in introducing a seton; but in most cases, a high degree of inflammation was excited, and in some this gave rise to very alarming symptoms. In all, it was far beyond what would appear necessary to the cure of the disease, although I took the precaution to remove the seton as soon as the inflammation commenced. In applying the seton, I first drew off the water, and then passed a long needle, with a curved point, and armed with a single thread of silk, up the canula, bringing it out through the skin of the tumour at about its centre, leaving the thread of silk *in situ*, loosely tying together the two ends. In these cases I have always visited my patients within six or eight hours after the operation. If the inflammation had commenced, I then withdraw the seton. But, even with this precaution, in some cases I have had suppuration of the sac; in one or two sloughing of the scrotum; and in all of them a degree of inflammation which required the strictest antiphlogistic treatment to subdue it. Mr. Key had a case in Guy's Hospital, in 1837, in which he passed a seton through a hydrocele without first drawing off the fluid. The patient died four days after from excessive inflammation. I have in some other cases adopted another plan of treatment. A patient was under my care who had had the fluid drawn off from a hydrocele seven times, and had been injected twice unsuccessfully with port wine. Eighteen months after the last operation he applied to me. He had then a large swelling on the right side of the scrotum; the transparency of which was very indistinct. I drew off 30 oz. of fluid, and then introduced through the canula a portion of elastic gum catheter. I then withdrew the canula, leaving the catheter within the tunic. This was worn for four days, when he began to complain of pain and redness. I then withdrew the catheter, and applied evaporating lotions to the scrotum. The patient perfectly recovered. In the case of a man in this Hospital, 56 years of age, the fluid was drawn off, and equal parts of port wine and water injected. In three weeks the hydrocele had again filled. I drew off the fluid a second time, and introduced through the canula a piece of gum catheter, as in the last case. By some accident the catheter escaped from the tunica vaginalis on the second day; nevertheless the patient was permanently cured. This mode of treatment, like the seton, is apt to give rise to excessive inflammation. I have known this to be the case in three or four instances, and have, therefore, since I have found the iodine injection so efficacious, entirely given up every other mode of treating simple hydrocele.—*Med. Times and Gazette, April* 17, 1852, *p.* 383.

DISEASES OF THE EYE AND EAR.

123.—OBSERVATIONS ON ARTIFICIAL PUPIL.

By William Bowman, Esq., F. R. S.

[Several cases specially impressed Mr. Bowman with the importance of a central position for the pupil, and he sought some means of enlarging the natural pupil to a limited extent in a suitable direction, still keeping it as central as possible, rather than of drawing it out with Tyrrell's hook to the margin of the cornea. It immediately occurred to him that the scissors of M. Luer possessed superior indications for this purpose.]

These scissors consist of delicate blades, about a quarter of an inch long, expanding from a stem which is moved up and down a canula of the size of a cataract-needle by means of a spring in the handle. When the spring is pressed, the scissors are closed by being drawn partly into the canula; and when it is relaxed, they open by being protruded by a spiral wire concealed in the handle. The blades of the scissors are pointed, and one of them projects beyond the other, and has a cutting-back edge; so that it is capable of piercing the cornea or sclerotica, and allowing the whole instrument (closed) to be introduced within the chambers of the eye. When within, the blades may be opened and made to cut the iris or capsule of the lens, by means of the spring in the handle. The canula and scissors are so adapted in size to each other, that the canula quite fills up the wound which the scissors make in the cornea, and thus the aqueous humor is prevented from escaping while the scissors are at work within.

With the view of adapting these scissors more perfectly to the operation for artificial pupil which I meditated, viz., to snip the border of the pupil at a given point without risking the lens,—I requested M. Luer to shorten the blades by almost one-half, and to make the shorter of the two blades blunt-pointed instead of sharp-pointed. I required the blades shorter in order that they might work in a smaller space, and one point blunt, in order that it might be passed behind the iris, and not puncture the capsule if it touched it. With this modified instrument, I operated as follows in the next case that presented itself.

Case 7.—Oct. 3, 1851.—Edward Bush, aged 36, lost the left eye by a blow three years ago; four months ago had severe inflammation with sloughing of the cornea in the right eye. At present there is a dense leucoma occupying the greater part of the right cornea. The iris is barely visible below, and the leucoma extends over the centre, obscuring the pupil. When the eye is shaded so as to dilate the pupil, it rises a little above the leucoma, and his sight is considerably improved. Still the cornea is slightly hazy above the leucoma, to nearly its upper margin. The lens appear to be *in situ*, and perfectly clear. The lower edge of the pupil adheres to the leucoma.

The patient being placed as usual on his back, I introduced the scissors at the outer side of the cornea, where it was very nebulous, and pushed them on as far as the existing pupil, where it lay almost, but not quite, obscured by the leucoma, for atropine had been applied. The shorter, blunt-pointed blade of the scissors was then passed behind the upper border of the pupil, and the long sharp-pointed blade in front of the iris; and, the spring being pressed, the blades closed and cut the upper margin of the pupil to the extent of about 1-16th of an inch. Though the hinder blade must have touched the lens, this was not wounded. No blood flowed to discolour the aqueous humour; and the man, sitting up, could at once see the trees and distant houses through the window. No inflammation ensued, and he returned to the country.

Early in November he came back for inspection, and his sight continued much improved; but as he stated that he saw more distinctly when the eye was shaded, and it was found that the pupil enlarged slightly upwards when that took place, I resolved to repeat the operation, and to divide the iris at the same point, but to a slightly greater extent, so as to place the pupil permanently in the condition in which it was thus found to serve most efficiently the purposes of vision. This was accordingly done, and the pupil enlarged by a second cut. The operation was exceedingly simple, the aqueous humour not escaping, and no bleeding attended it. He left the Hospital two days after. A minute strip of iris remained between the two cuts which had been made in it. When this shrinks, the man's sight will doubtless be *much improved*.

No inflammation or opacity of the lens followed this second operation, which shows how much may be done with safety with this delicate instrument.

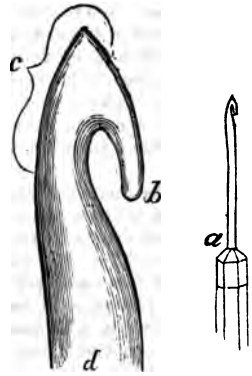
These scissors may be used also with advantage through the cornea to make an opening in the iris when the pupil is obliterated by prolapse, after the extraction of cataract; and indeed, I think, that while they enable us to make the pupil as central as possible, to define its extent, and to save the lens when present, and all this with a mere puncture of the cornea, they must supplant altogether the iris scissors of Maunoir, which require a large incision in the cornea, do not enable the surgeon to limit the pupil as he desires, and are very apt to wound the lens, induce cataract, and mar the elegant results of the operation as produced by the canula scissors.

Mr. Tyrrell employed his hook to make an artificial pupil in some cases of conical cornea, an operation which has been abandoned probably by most surgeons, on account of the inconveniences already detailed as attendant on the use of this instrument, inconveniences which must have been even greater where the pupillary border was entirely free, than where it was partially adherent to a leucoma. I should not now hesitate, in a suitable case of conical cornea, to introduce the canula scissors, and to incise the pupillary border at such a point as to enlarge the pupil by the resulting gaping of the incision, opposite a part of the cornea near the centre, which was more fitted than the irregular apex of the cone to transmit a true refraction to the lens and retina.

I would also recommend the use of these scissors to enlarge the pupil where the cornea is thickly nebulous, not leucomatous, and where vision is much improved by dilatation of the pupil. The constant use of atropine greatly improves sight. By snipping the pupillary border at two or three points, and to a slight extent, the pupil is permanently enlarged, but at the same time kept central. The operator need not hazard the lens, and the danger of inflammation is trivial. Such cases are numerous, and have not admitted of relief by any operation hitherto devised. The operation would be performed much as in Case 7. The scissors introduced below might snip the outer and inner margin by being turned to each in succession.

I may add, that these scissors are also extremely useful in cutting across bands of opaque tough capsule, which are often very hard to deal with by the ordinary cutting needle. This use of them is exemplified in Case 2.

Notwithstanding what has been above advanced as to the disadvantages of the operation with Tyrrell's hook under ordinary circumstances, there will remain certain cases in which some such instrument will be useful. For example, where the whole cornea is leucomatous except one small marginal spot, the pupil must be made behind this spot, and the smallness of the spot will limit the pupil, so that a large opening in the iris would be of little consequence. Again, in cases, where with synechia posterior, a small point of the pupillary border remains free, it will be desirable to hook it and draw it outwards, so as to make the elliptical pupil, which has been shown to be so serviceable. Here the iris readily tears rather than the adhesion to the capsule should give way, and the hook answers well; nor could the scissors be employed.



a, size of needle-hook. *c*, cutting edge.
b, blunt-hook (enlarged view.) *d*, stem.

But, to obviate the defects already alluded to, even in cases to which a hook may still be applicable, I have contrived the annexed needle-hook, which has been made for me by Mr. Weiss. (See Fig on previous page.)

Of the general size of Tyrrell's hook, it is sharp and flattened at the point, and the stem is cylindrical, and of such a size as exactly to occupy the corneal wound, and effectually prevent the escape of the aqueous humour during the operation. There is a slit on one margin near the point, running up towards the point, and making the terminal part of the instrument a hook as well as a needle. The cutting edge extends from the point as far as the shoulder on the side opposite the slit; while, on the other side, or that which must be turned towards the lens in seizing the pupillary border, the edge is cutting only close to the point, and the convex part of the hook, that might touch the capsule, is blunt.

With this instrument, no previous puncture with the needle is requisite. The needle-hook introduces itself, retains the aqueous humour, and brings out the iris, while it is calculated to insure the safety of the lens as much I believe as Tyrrell's hook. In fact the retention of the aqueous humour by maintaining the exact position of the iris and lens during the surgeon's manipulations, must itself go far to enable him to avoid wounding the lens.

The slit may be made of different depths in different instruments, so as to entangle more or less of the iris as desired; and the recess of the slit is cutting in one, for those cases in which it is desirable to slit up the iris from the pupillary border rather than to withdraw or remove a portion with the scissors. In the latter cases, when the iris has been slit up as far as necessary, say one half of the distance from the pupillary to the ciliary margin, the hook may be disengaged and withdrawn, leaving the pupil much limited in the direction of the ciliary margin.

If it should happen to seem desirable to re-introduce the hook after a first attempt to withdraw a portion of iris, Tyrrell's hook will still be the best instrument, and may be used as a supplementary aid to the needle-hook, which manifestly is not adapted for introduction a second time by the same aperture.

In conclusion, I have pleasure in appending a very interesting case, which has been placed at my disposal by my friend Mr. W. W. Cooper, in which he very ingeniously made an excellent pupil by means of another of the new French instruments. It is as follows:—

Samuel Hill, of Derby, aged 45, a stout, florid man, by occupation a porter, was attacked in February, 1850, with iritis of both eyes, which ultimately led to the closure of the pupils by deposit of lymph. They were contracted to the size of pins' heads, and the iris was much stretched in each. In the lower part of the left pupil there was a clear point, but so small as to be scarcely perceptible.

This man became a patient at the North London Eye Infirmary, and was operated on by Mr. White Cooper, on the 25th of last October.

The right eye was selected as being the worst, and the operation performed was as follows:—A small incision was made with Jäger's double-edged knife through the outer margin of the cornea, a little below the median line; the aqueous humour escaped, but the iris did not bulge into the wound. A pair of extremely fine forceps (made by Luer after the pattern of Charrière) were then introduced, with the blades closed; when in front of the pupil the blades were opened by means of a spring, and a few fibres of the iris at the external border of the pupil seized, and very gently drawn out of the wound. The rent thus made in the tense membrane immediately dilated, forming a good-sized oval pupil. Nothing more was required; not a drop of blood appeared, and the eye was closed and bound up.

The patient recovered without a bad symptom, and returned home to Derby on the twelfth day after the operation, having regained good vision.

This is probably the first time in which these forceps, intended for the removal of opaque portions of capsule, were used for the formation of an artificial pupil. The wound in the cornea did not exceed an eighth of an inch in size, for the peculiar construction of the forceps enabled them to be expanded when in the anterior chamber. The blades grasped the iris where it was firmly bound down to the capsule of the lens, but without injury to that delicate membrane; and the small portion was withdrawn without any other part of the iris having been touched, much less bruised—a point of great importance in such cases.—*Med. Times and Gazette, Jan. 10, 1852, p. 33.*

[In a subsequent number of the 'Medical Times and Gazette,' Mr. Bowman observes that he was not aware at the time he spoke of the canula scissors above mentioned, that Mr. Wilde of Dublin had previously described a similar instrument in the 'Medical Times' of Dec. 7, 1850, under the name of a new scissors for the removal of opaque capsules and false membranes from the chambers of the eye.]

124.—*Case of Nævus of the Eyelid, Cured by Platinum Wire heated Red-hot by Galvanism.*—By RALPH M. BERNARD, Esq., Clifton.—[In this case a child 5½ months old was admitted an in-patient at the Bristol Eye Hospital, Oct. 25, 1849, with nævus of the upper eyelid of the right side; various remedies had been resorted to, but with no good effect. Mr. Bernard describes the operation as follows:]

Two pieces of thin platinum wire were passed at right angles to one another, through the base of the nævus, and heated red-hot, for the space of rather more than half a minute, by connecting them with a galvanic battery; the wire was then withdrawn, and cold-water dressing applied. Small superficial sloughs formed at the points where the wire penetrated the skin, but healed in about three weeks.

The tumour was much diminished in size, and I considered that, after a little time, the remains would have been obliterated; however, on April 17th, 1850, as the swelling had increased on each side of the part of the lid which had been treated with the red-hot wire, I proceeded to introduce four platinum wires at right angles, heated as before, with results perfectly satisfactory, as the present appearance of the child is as follows:—Tumour quite obliterated; skin of eyelid rather less moveable than the other side, and showing six small cicatrices, which, however, are not visible at the distance of two yards.—*Med. Times and Gazette, March 27, 1852, p. 318.*

125.—*Diluted Hydrocyanic Acid as a Topical Application in Certain Affections of the Eye.* By JAMES VOSE SOLOMON, Esq., Surgeon to the Birmingham Eye Infirmary. [Mr. Solomon was induced to try this remedy, from having seen it applied in the Westminster Eye Infirmary in every stage of strumous ophthalmia, by Mr. Charles Guthrie. Mr. Solomon says:]

One part of Scheele's prussic acid, diluted with two parts of distilled water, has appeared to me in general to be the best formula for use, though, in some instances, greater concentration or dilution may be required. When a drop of dilute prussic acid of the first-named strength is applied to the conjunctiva of a healthy eye, there follows a sense of slight uneasiness and warmth, with congestion of the vessels of the part, lasting for a few minutes; the pupil is slightly dilated. If a more concentrated preparation, say equal parts of Scheele's acid and distilled water, be used, there will be, in addition to these symptoms, aching pain in the eye-ball.

The pain attendant on the external ophthalmiæ in their acute stage is generally aggravated by the application of even the most diluted form I have mentioned, though, in some few instances, it has not produced such effect. It is when the acute stage has been subdued by appropriate treatment, or in cases where the symptoms of irritation are greater than those of vascular excitement, that the diluted hydrocyanic acid proves to be of singular benefit. A lingering chronic stage, with the liability to relapses, is averted; dimness of vision, intolerance of light, and lachrymation, quickly yield to its calmative powers.—*Med. Times and Gazette, Feb. 21, 1852, p. 185.*

126.—*On Ung. Picis in Ophthalmia Tarsi.* By DR. PARRISH.—Dr. Parrish believing the obstinate disease *ophthalmia tarsi* to be analogous in its nature to *tinea capitis*, in which *ung. picis* proves of such great service, has resorted to it in the former affection with almost undeviating success. It is carefully rubbed into the roots of the *tarsi* at bed-time, and washed away with castile soap and tepid *water in the morning.—Amer. Jour.*—Brit. and For. *Medico-Chirurgical Review, Jan., 1852, p. 274.*

127.—*Iron Spiculæ in the Cornea.* By Dr. Henry Jeanneret.—The difficulty of extracting spiculæ of iron or steel imbedded in the cornea or conjunctiva,—an accident so frequent among engineers, is often so much more embarrassing than one expects, that I am sure those of the profession who are not aware that the use of the knife may always be avoided, will be glad to recollect, that by repeating one of the simplest of their experiments in chemistry,—the immersing a piece of iron in solution of sulphate of copper, the iron is entirely removed. I usually use the collyrium of one to three grains to the ounce. Although I have mentioned it to many, I never saw the process referred to in print. I was first led to adopt it in my own person when abroad some years ago. While turning a steel cylinder, a point flew off, and stuck fast in the cornea, producing much pain. A professional friend having made several endeavours to extract it, was about to make an incision, when the thought struck me, and, by keeping the eye open in a wine-glassful of the solution, I was speedily relieved from all uneasiness.—*Med. Times and Gazette, April* 24, 1852, *p.* 428.

128.—ON THE TREATMENT OF POLYPI OF THE EAR.

By Joseph Toynbee, Esq., F.R.S.

Polypoid excrescences are not uncommonly met with in the external meatus, and they are generally the result of long-continued irritation of its dermoid layer. As a general rule, polypi of the ear are attached to the membranous meatus, although cases are sometimes met with in which they spring from the outer surface of the membrana tympani, and in one dissection I found a polypus growing from the inner surface of the latter organ. Polypi are always attended by an abundant discharge of mucous fluid, which often has so offensive an odour that the patient is obliged, as much as possible, to avoid society. This affection is generally attended by very little uneasiness in the ear. At times there is a sensation of fulness and pressure; but one of the most prominent and urgent symptoms is a sense of heaviness, which is very frequently accompanied by giddiness, and a feeling of confusion in the head; and sometimes there is a shooting pain, which extends from the ear to the temple. These head symptoms, which are often very distressing to the patient and alarming to his friends, appear to be produced by the pressure exercised by the polypus on the outer surface of the membrana tympani. This pressure on the outer extremity of the chain of tympanic ossicles produces a movement inwards towards the vestibule of the inner extremity; for upon careful examination of a specimen prepared for the purpose, it is manifest that, although there are two articulations between the long process of the malleus and the base of the stapes, the slightest movement of the processus longus mallei inwards causes the base of the stapes to be pressed inwards towards the cavity of the vestibule. After the observation of many cases in which head symptoms have been associated with affections of the ear, I have come to the conclusion, that pressure upon the contents of the vestibule may produce—1, a sensation of noises; 2, A feeling of giddiness; 3, Confusion of ideas. That these symptoms are produced by pressure on the contents of the vestibule, may be ascertained from the examination of cases in which collections of cerumen occur in the external meatus; for not unfrequently the medical man meets with instances in which continued noises and giddiness are present to so great an extent, that the patient is often obliged to stop when walking, and hold himself by the nearest object; and these symptoms wholly vanish immediately that the accumulation is withdrawn from the outer surface of the membrana tympani.

A careful examination of the ear by means of a speculum and a strong light, is at once sufficient to determine whether a polypus is present, premising that the ear has been carefully syringed with warm water so as to remove all discharge.

Polypi of the external meatus may be divided into three classes:—

1. The one of most frequent occurrence, and which may be called the vascular polypus.

2. That which has been termed the gelatinous polypus.

3. One that has not hitherto attracted the attention of surgeons, and which may be styled the globular vascular polypus.

1. The *Vascular Polypus* is of a red colour, plentifully supplied by vessels, and so soft that, upon being taken hold of by a pair of dressing forceps, it breaks up and blood escapes from the lacerated surface. The vascular polypus rarely increases to so large a size as to dilate the meatus; it generally grows from the wall of the meatus, about midway between its outer orifice and the attachment of the membrana tympani. It is composed of small rounded cells, and its surface, which is sometimes covered by ciliated epithelium, is very smooth and shining. This polypus rarely extends further than the orifice of the meatus, where it can often be seen without the use of any artificial means; frequently it is confined to the inner half or two-thirds of the meatus. It is not uncommon for the vascular polypus to remain during several years throwing off its offensive secretion, without producing symptoms of a nature sufficiently urgent to induce the patient to apply for relief; in other cases, the head symptoms soon become so distressing as to cause serious alarm.

The treatment generally adopted of applying astringent lotions and drops, or of touching the surface of the polypus with the solid nitrate of silver, has, in my hands, been quite useless; and so far as my experience has extended, all attempts at extraction fail, because the polypus breaks up immediately that any force is applied to it, and it again rapidly grows to its former size. After having been long baffled in the treatment of vascular polypus, and having tried by a great diversity of applications to cause its disappearance, I resorted to the use of the potassa cum calce, and thus far my success with it has been sufficiently great to induce me to recommend its use to the profession. I will proceed to describe the mode in which it has been applied. In the first place, it is important that the substance used should be made into very thin sticks; those supplied to me by Mr. Squire as recommended in a paper by Dr. Hy. Bennet, answer the purpose extremely well, so long as they retain their size and form; but as this substances deliquesces very rapidly it is important that the greatest care be taken to keep it excluded from the air. For use at St. Mary's Hospital, the potassa cum calce has been supplied by Bailey, of Wolverhampton;* and it differs from that I had previously used in containing a small quantity of iron, which addition makes it firmer and harder, and it deliquesces much less rapidly than when prepared in the usual way. Perhaps the latter preparation, in not requiring so much care in its application, is to be preferred for hands unaccustomed to its use; but the one supplied by Mr. Squire is certainly the most efficacious.

In the application of the potassa cum calce care is requisite so as to avoid touching the surface of the meatus; it is so extremely sensitive, that the pain produced by the action of an escharotic upon it is very acute. The polypus, on the contrary, possesses little or no sensibility; if, therefore, the application be carefully made, the operation is not attended by any pain. The mode of proceeding which I am in the habit of pursuing, is as follows:—The external meatus having been syringed with tepid water, so as to remove the whole of the discharge from the surface of the polypus as well as from that of the meatus, the tube and polypus should be dried by a portion of fine cotton wool attached to the end of a probe. A portion of glass tube, about an inch and a half long, should then be selected, and care ought to be taken that it is so embraced by the meatus, that it is not liable to be shifted from its position by any movement of the head of the patient. This portion of glass tube is to be introduced into the meatus, and passed inwards as far as the polypus, when, by a gentle pressure, a portion of the free extremity of the polypus is made to protrude into the interior of the tube, and is surrounded by it. Upon looking into the tube, and ascertaining that the polypus is embraced by its inner extremity, the tube is steadied by the left hand, and with the right a portion of the potassa cum calce is passed inwards, and gently pressed against the polypus. If a pair of rectangular forceps be used, neither the hand of the surgeon nor the instrument he employs prevents the operator from seeing the polypus while he is making the application, and he can, therefore, be sure that he touches the whole of that

* *The large sticks* sent by Bailey were re-cast into smaller ones by Messrs. Hopkin and Williams, of *New Cavendish-street.*

part of the growth which is in the tube.* The immediate effect of the application of the potassa cum calce upon the surface of the polypus, is to change its colour from a bright red to a livid hue; and this takes place without any pain being experienced by the patient, if the meatus has been completely guarded. After the application has been made, the patient should sit still for three or four minutes, and the tube be allowed to remain as it was fixed during the operation.

Upon inspecting the polypus at the end of these minutes, it will be found to have changed to a dark purple colour, to have blood oozing from it, and, instead of its former rounded extremity, it presents an uneven, pulpy mass. The meatus is now to be syringed out with tepid water, when blood, mixed with the *débris* of the polypus, will come away; the surface of the polypus still remains of a dark colour, and, during several hours, a process of slow dissolution takes place in all that portion which the escharotic has reached. There are two other directions that it is important to bear in mind. 1. The surface of the polypus is sometimes rendered so dry by means of the cotton-wool, that there is not sufficient moisture to cause the escharotic (especially when containing iron) to deliquesce; the point of the potassa cum calce is, under these circumstances, to be slightly moistened. 2. Should the patient experience pain at the time of the application, or during the few minutes afterwards, the ear should immediately be syringed with tepid water, —the effect of which is at once to remove all painful symptoms, and to arrest the action of the escharotic.

Previous to detailing some cases of vascular polypus in which the potassa cum calce was used to destroy the diseased growth, it will be well for me to make a few further observations respecting the treatment usually adopted in such cases by extraction, or rather attempted extraction. The disadvantages belonging to the attempt at a removal of this disease by extraction are:—

Firstly. The impossibility of removing the whole of the mass on account of its soft texture, and the destruction of a portion only is followed by its rapid regeneration.

Secondly. In many patients the vascular polypus co-exists with, and is indeed symptomatic of, more deep-seated disease in the bone or brain, and any attempts at removal, by using force, are liable to originate an irritation of the bone, which may extend to the brain and rapidly terminate life.

In cases where the polypus reaches nearly as far as the outer orifice of the meatus, it is desirable that a portion of the diseased growth should be removed by a pair of curved scissors, as recommended by Mr. Wilde, or by the use of the snare, as described by the same gentleman. It is better, however, not to attempt to remove the growth by cutting through the roots, inasmuch as, after this proceeding, an escharotic must be applied to ensure the entire destruction of the mass. It will be remembered, that the roots of the polypus are very sensitive, and the application of the potassa cum calce to them is productive of very acute pain; and what is still more important, it is very difficult to keep the escharotic from touching the meatus, and inflammation, and even ulceration, of this delicate structure may arise from such an occurrence. The great advantage which will be found to accrue from the application of the potassa cum calce is, that it not only destroys the portion of the polypus to which it has been applied, but after a few applications it seems to overcome its tendency to increase, and ultimately to cause the roots to disappear without the escharotic being applied to the roots themselves.

Vascular Polypus near the Membrana Tympani.—Singing in the Ear.—Power of Hearing Diminished.—Treated with Potassa cum Calce.—Cure. *Case* I. Mr. W. L., aged 40, was sent to me on the 21st July, 1850, by Mr. Cook, of Guy's Hospital. He was a large, strong man, and in good health. He states, that five or six years ago he found that he was dull of hearing in the left ear, this dulness was removed by the use of the syringe, which brought away a considerable quantity of cerumen. He remained without cause of complaint until three months ago, when the same symptom returned in the left ear. He was again in some measure relieved by syringing, but this operation was followed by a discharge of a very

* In the uncertain atmosphere of London, I use Mr. Avery's lamp held between the teeth, and thus leave both hands at liberty; but a good sun light, or even the bright light of a fine day, is quite sufficient.

offensive character, attended by a sensation of singing in the ear. He has at times complained of slight pain in the ear. Upon examination the meatus was found to be full of matter, which, after being removed, was seen to consist of adhesive mucus, mixed with epidermoid cells, the latter giving a milky aspect to the matter. The former presented numerous small flocculi, or rather little particles of cotton-thread, from a line and a half to three lines in length. Upon examination of the meatus after this mucous fluid was removed, a polypus of a red colour was observed. It was situated deep in the meatus, of which it apparently occupied the inner fourth, entirely obstructing a view of the membrana tympani. The power of hearing was much diminished,—the watch which I have some years used as a test of the hearing of patients not being heard, except when pressed upon the outer ear. From the treatment of similar cases, I felt that it would not be desirable to attempt the removal of this mass by instruments, not only on account of the soft texture of the polypus, and from its extreme sensitiveness, but also from the uncertainty as to what part the roots of the polypus were attached; and I felt that if it were fixed to the membrana tympani, the use of any force might prove injurious to that organ. Having since ascertained, by dissections, that the dermoid layer of the meatus is continuous with the dermoid lamina of the membrana tympani, it appears desirable, on this account also, not to use any force to a polypus attached to the vicinity of the membrana tympani, as a laceration of the meatus is likely to extend to the membrana tympani.

July 21.—The potassa cum calce was applied to the surface of the polypus in the way previously described; in the course of a minute, the whole of that part of the polypus which could be seen became of a livid hue, and, when the ear was syringed with tepid water, five minutes afterwards, numerous small particles of the polypus, softened and disintegrated, were brought away. The patient complained of but very slight pain, and this entirely disappeared upon the use of the syringe.

July 29. The polypus is smaller and less red; the discharge from the meatus very copious. The potassa cum calce was again applied, with similar results.

Aug. 13. The polypus is observed to be much smaller. Instead of the mass which, at the first examination, had an appearance like a raspberry, there was now only a rounded button, about a line and a half in diameter. The potassa cum calce was again used.

Oct. 18. The discharge is much diminished; its odour is less fetid; polypus nearly gone; the power of hearing is much improved.

Nov. 13. Since the last visit, a lotion, composed of a drachm of tannin to eight ounces of water, has been used twice daily; the discharge has now nearly disappeared, and consists almost entirely of epidermoid cells. The only vestige of the polypus consisted in the presence of two very small buttons, each of the size of a small pin's head; these were attached to the upper part of the membranous meatus, close to the membrana tympani.

Nov. 29. The use of the injection has been continued; the polypus has entirely disappeared; the singing noise has ceased; the surface of the membrana tympani is dull. Hearing distance of the watch is six inches.

The features of interest in this case appear to be: Firstly, that, although the escharotic was only applied at intervals of several weeks, the polypus did not increase in those intervals, but, on the contrary, it gradually diminished. Secondly, With the destruction of the polypus, the singing entirely disappeared. Thirdly, The return of the power of hearing to so considerable an extent.

Discharge from the left ear during many years—Giddiness.—Polypus; treated by potassa cum calce.—Cure. *Case* II. The Rev. H. C., aged 40, consulted me on the 21st April, 1851. His constitution was not robust, he having spent some time in India. The history of his case is, that the left ear has been deaf from childhood, that since early life he has been troubled by a discharge of offensive matter from that ear, and that lately he has complained of attacks of giddiness, especially upon rising suddenly from his chair. Upon examination a red polypus was observed to fill the inner half of the meatus. The same treatment was adopted in this case as in the last, with the exception that, as the patient lived in the country, he had the potassa cum calce applied two or three times in the space of nine or ten days, and then a longer interval elapsed before it was again used. The same result, however,

ensued, and at the end of six months the polypus had disappeared, and the symptoms of giddiness wholly vanished.

In another case, that of a patient, aged 26, where the polypus filled the whole of the meatus, portions were removed by forceps, and the remainder destroyed by potassa cum calce. The symptom of interest in this case was the production of giddiness by pressure on the outer part of the polypus.

In cases of polypus, which co-exist with catarrhal inflammation of the dermoid meatus, it is desirable to keep up a slight counter-irritation by vesicating paper over the mastoid process, while the polypus is being treated by the potassa cum calce. In order to remove the unpleasant odour of the discharge, an injection composed of one part of the solution of chloride of lime to twelve parts of water, may be used three or four times daily.

[Next in frequency of occurrence to the vascular is that of the *Gelatinous Polypus.* This growth generally increases to a large size, as large as that of a small nut,—attached near the root are generally to be found a number of smaller growths, which appear to be smaller polypi impeded from further enlargement by the presence of the larger one. The interior of the gelatinous polypus is composed of corpuscles and fibrous tissue; the latter predominating. These corpuscles are generally circular, sometimes approaching to an oval shape, and the size being about equal to, or one-half, or even one-quarter, the magnitude of the blood corpuscle. The single fibres are extremely fine, the diameter varying from one-half to one-quarter that of the blood disc. This kind of polypus sometimes becomes exceedingly hard, from the increase of the fibrous matter, and the diminution of the corpuscles, and in the absence of the gelatinous matter between these.]

Treatment of the Gelatinous Polypus.—The difference in the structure of the two kinds of aural polypi naturally prepares the surgeon to expect that the treatment requisite for their removal would also differ. This is undoubtedly the case. The use of the potassa cum calce, which has proved of so great value in the destruction of the vascular polypus, is of but little service in the treatment of the gelatinous, or, more properly speaking, the fibro-gelatinous polypus. The escharotic produces but comparatively slight effect upon fibrous tissue, and the only plan of removing it is by extraction. For this purpose, the best instrument is a pair of ordinary dressing-forceps, the ends of which should be reduced in size, so as not to be larger than from a line to a line and a half in diameter. These forceps should be introduced into the meatus to the distance of half or three quarters of an inch, and the polypus seized as near as possible to its roots; the forceps should then be used as a lever, the outer part of the ear being the fulcrum, and the polypus turned out of the cavity. But little force is required, and, as a general rule, the diseased growth is removed without difficulty in an entire state. Upon examining the meatus after its removal, the surface to which it was attached is distinctly discernible, and, for a short time, there is a slight oozing of blood from it. In some cases portions of the root of the polypus remain, but they do not generally require any further treatment, but gradually atrophy and disappear. On the contrary, if any of the small globular bodies remain attached to the root, they rapidly increase, and the diseased growth has again to be submitted to operation. The removal of the fibro-gelatinous polypus is generally productive of relief, not only to the unpleasant head symptoms, which are caused by its pressure on the contents of the vestibule, but to the diminished power of hearing. The improvement in the power of hearing does not, however, as would be supposed, take place at once; on the contrary, it not uncommonly occurs that there is, at first, no increased power of hearing, but that it gradually and very slowly improves. This may, perhaps, be accounted for, from the circumstance, that the polypus has for a long period exercised considerable pressure on the membrana tympani, or where that structure no longer exists, upon the tympanic ossicles, and that these organs only slowly return to their natural state.

The third kind of polypus developed in the external auditory meatus is the *Globular Vascular Polypus.* I have given this name to a growth which differs considerably from those comprised in the two preceding classes, in consisting of a single globular mass, perfectly smooth on its surface, without any appearance of granulations; it is confined to the inner fourth or sixth of the meatus, from the upper part of which it is usually developed, and it hangs down like a curtain,

wholly or partially concealing the membrana tympani. It is of a deep red colour, and softer than the common vascular polypus, and it does not generally increase to a larger size than an ordinary pea. This growth generally occurs in children or young persons; it is attended by a mucous discharge, which is generally offensive, and, like the secretion accompanying the other forms of polypus, it consists of epidermoid cells, which give a milky appearance to the water, and also of fine threads of mucus. This kind of polypus may exist several years without producing any severe symptoms; it has hitherto been comprised under the general term "otorrhœa." It may be distinguished from the disease I have elsewhere called chronic catarrhal inflammation of the dermoid meatus in the discharge containing flocculi of mucus, like small particles of thread, and upon its always presenting a red mass at the inner extremity of the meatus. This affection also differs from catarrhal inflammation of the meatus, in not terminating in disease of the bone, the discharge apparently proceeding from the surface of the polypus only, to which part the disease is limited. The affection of the ear with which the globular vascular polypus is most liable to be confounded, is catarrhal inflammation of the mucous membrane of the tympanum. In some of these cases, the mucous membrane of the tympanum is of a deep red colour, and so much tumefied that it projects into the meatus for a line or a line and a half beyond the position occupied by the membrana tympani previous to its destruction. Upon examination of the growth by means of a speculum and lamp, it is not easy to determine which of the two diseases above noticed is present. The examination of the discharge is, however, sufficient to decide the question; although in both affections there is floccular mucus present, that accompanying the polypus is composed of small thread-like particles, while that emanating from the mucous membrane of the tympanum presents large, irregular-shaped masses, having generally a yellow colour. The history of the case will also generally aid the surgeon in forming a diagnosis; the vascular globular polypus generally appears without the manifestation of any very decided symptom; perhaps the appearance of the discharge is the first indication of its existence; whereas the affection of the tympanum generally originates in an attack of acute inflammation, and it often arises during scarlet fever or measles.

Treatment.—The treatment of this kind of polypus is much more simple than that of the two species which have been already described; nevertheless, unless the true nature of the disease be ascertained, it is useless to attempt to combat it by the use of the astringent applications ordinarily prescribed to arrest discharges from the ears. This affection stands between the ordinary vascular polypus and catarrhal inflammation of the dermoid meatus; the former is wholly uninfluenced by the use of the strongest astringents; the latter is generally curable by weak solutions of them; the vascular globular polypus is affected by astringent applications, but they require to be of considerable strength. The course of treatment which I am in the habit of following consists in having the meatus of the affected ear syringed out with tepid water, so as to remove all discharge, and after the ear has been turned towards the shoulder of the affected side, to allow of the water running out, three or four drops of an astringent solution are to be dropped into the meatus, and the ear closed for half an hour by a portion of cotton-wool moistened with it. This course may be repeated twice, thrice, or oftener, during the day, care being taken that the sediment from the solution be removed before the drops are again used. The preparations I have used are the acetate of zinc, the acetate of lead, alum, and tannin; but that first named has, I think, answered best. This species of polypus may often be removed in the course of a week, or from that to a fortnight; and to prevent any congestion in the tympanic cavity, a slight discharge from the surface of the mastoid process has generally been kept up during the time that the astringent solution has been used.

Globular Vascular Polypus.—Discharge for three years.—Cured by a Solution of Liquor Plumbi. Case VII. Miss F. A., aged 12, of a rather weakly constitution, was brought to consult me on the 30th of March, 1850. Her mother stated, that she had been dull of hearing during several years in the left ear, and this dulness has of late so much increased, that when the right ear is pressed upon the pillow she cannot hear even loud voices. During the last three years there has been a discharge from the ear, which has sometimes been very offensive, and it has been at times of a dark colour, especially in a morning. There has been no complaint

of pain in the ear, but a tenderness has been experienced below it. At times there has been pain over the left eyebrow, which has also extended to that side of the head. Upon examination, it was found that the watch (of which the natural hearing distance was three feet) was heard only when placed in contact with the ear. By the aid of the speculum, a globular red growth, like a polypus, was observed to conceal the membrana tympani, with the exception of a semi-lunar-shaped part at its inferior margin, about half a line in diameter at its centre, which was quite opaque. As this growth did not extend far into the meatus, as it was of a deep red colour, and its surface quite smooth, it presented an appearance very analogous to that of the thickened mucous membrane of the tympanum, which, in some cases, becomes so much hypertrophied as to project into the meatus, and occupies a position nearer to the orifice than did the membrana tympani previous to its destruction. The presence of the membrana tympani was, however, ascertained by the use of the otoscope, air was heard to enter the tympanic cavity, and it did not pass into the meatus; the small portion of the membrana tympani visible, was observed to be pressed out and rendered tense and more white when the tympanic cavity was filled with air. It was therefore evident that the diseased growth was a polypus.

The treatment pursued consisted in applying to the surface of the polypus thrice daily a solution of the diacetate of lead; and, as I was not to have the opportunity of seeing the patient for between two and three months, it appeared desirable that it should not be so strong as it would otherwise have been advisable to use. The drops were composed of six drops of liquor plumbi to an ounce of water.

June 18.—The discharge has nearly disappeared, and, upon examination, the polypus is reduced to the size of a large pin's head. The hearing was improved, the hearing distance of the watch being two inches, instead of being heard only when in contact.

I had not an opportunity of seeing this patient again, but some little time after the last visit I heard that the discharge had ceased, and the hearing had so much improved that she was considered cured.

Globular Polypus in Right Ear.—Broken up by Forceps, and a Solution of Liquor Plumbi applied.—Cure. *Case* VIII. Master A. H., aged 16, was first seen by me on the 13th of April, 1850. Both tonsils were enlarged; he had a tendency to glandular enlargements; and was at the time of his visit far from being in strong health. The history of the case was as follows:—At six years of age he suffered from an attack of scarlet fever. Subsequent to it, he was dull of hearing, and this dulness has increased during the last two years and a half; has had discharge of an offensive character from the right ear during the last eighteen months.

Right Ear.—Upon inspection, a polypus was observed at the inner extremity of the meatus, growing from its upper part near to the membrana tympani; this polypus was red, globular, and having a smooth, shining surface. The discharge, upon being removed from the meatus, was observed to be white, like milk—not viscid; and it consisted of rounded cells, similar to those excreted in catarrhal inflammation of the mucous membrane of the tympanum; the nuclei of these cells were rendered very distinct by the addition of acetic acid. By aid of the otoscope, air is heard to enter the tympanic cavity, and in so doing, to produce a loud sound, like that attendant upon the sudden distension of a bladder by air. The watch was not heard over the ear; it was heard when pressed on the temple.

Left Ear.—The membrana tympani was dull on its surface and white. Air passed into the tympanic cavity, producing a sound similar to that in the right ear. Hearing distance a quarter of an inch.

April 17.—By means of the rectangular forceps the outer part of the polypus was broken up; it was very sensitive and bled slightly. A solution of diacetate of lead in water (four grains to the ounce) was ordered to be dropped into the ear thrice daily, and a cantharidine cerate was applied to the nape of the neck.

25th.—The discharge much diminished, and its offensive odour has disappeared; the polypus is diminished to one-third of its former size, and the membrana tympani is seen beyond its lower border.

May 4.—Much the same; prescribed a solution of chloride of zinc (ten grains to the ounce) to be dropped into the ear.

16th.—The discharge has disappeared; hearing so greatly improved that he thinks he sometimes hears quite well; the roots of the polypus only remain. This treat-

ment was continued, and when I last saw the patient, on the 13th of September, he was quite well.

In the following case I adopted a rather more active plan of treatment. Being aware from actual examination that polypoid growths of the nature now under consideration are extremely soft, in order to remove them more rapidly I resolved to apply to them an astringent of much greater strength than those previously used, and the complete success of this proceeding was very manifest.

Globular Vascular Polypus. Destroyed by a strong Solution of Acetate of Zinc. Case IX. Miss T., aged 21, of fair complexion, and in good health, applied for advice on the 13th of January, 1852. The history of the affection is, that seventeen years ago, after an abscess behind the right ear, its hearing power considerably diminished and has never returned. Six months ago discharge issued from this ear, and has continued to do so up to the present period without any intermission. Upon examination of the *right ear*, the watch was only heard when in contact; the meatus contained a considerable quantity of discharge, which was found to consist of tenacious thready particles of mucus and mucous cells. At the inner extremity of the tube was a globular shaped polypus of a red colour; it was attached to the upper part of the membranous meatus close to the membrana tympani, which it wholly concealed, with the exception of a small semi-lunar shaped portion seen at its lower border.

Left ear.—Hearing distance one inch. Membrana tympani white, like cartilage.

Treatment.—The right ear to be syringed out with tepid water thrice daily, and after each operation four minims of a solution of acetate of zinc (forty grains to the ounce) to be dropped into the ear. A small portion of vesicating paper to be kept applied over each mastoid process.

Jan. 15.—There was slight pain for half an hour after the first three applications of the drops, and yesterday the patient complained of a sensation as if the ears were distended. The discharge is diminished in quantity. The polypus had a greyish colour, bled upon being touched, and appears partially broken up. Large particles of the acetate of zinc had collected on the surface of the polypus.

19th.—Polypus smaller; no discharge; has had considerable aching in the ear. To syringe with warm water, and to omit the use of the drops.

22nd.—The pain has ceased; there is no discharge; the polypus has wholly disappeared. The membrana tympani is now seen; its upper and posterior part is white and thick; at its anterior part there are two small apertures, through which the mucous membrane of the tympanum is seen to be red and thick; the hearing has improved; the watch is now heard by the right ear at a distance of three inches, by the left at a distance of two inches. Subsequent reports from this patient state, that the discharge has not again returned, and that the hearing continues to improve.

Conclusion.—In reviewing the foregoing pages on the nature and treatment of the different kinds of polypoid growths in the external meatus, it is evident that no effectual plans can be carried out for their removal, unless a careful diagnosis be first made, as the remedies which will effectually remove one species of polypus are found to have no influence upon others. This diagnosis is made without difficulty by means of a lamp and speculum after the use of the syringe. The lamp which I should recommend to the surgeon when he sees patients from home, is the efficient and inexpensive one made by Mr. Miller, of 179, Piccadilly. It does not require the use of gas or fluid; it is very portable, and of use not only for the ear, but for the eye, mouth, and throat—indeed for any purpose for which a strong light is required. When the surgeon is seeing his patient at home, and can have the advantage of gas, Mr. Avery's lamp is to be preferred.—*Med. Times and Gazette, Jan.* 3, *p.* 14; *Jan.* 31, *p.* 105; *Feb.* 28, *p.* 212; *and April* 3, 1852, *p.* 343.

129.—OTORRHŒA WITH PERFORATION—DESCRIPTION OF A NEW DIAGNOSTIC SYMPTOM.

By W. R. Wilde, Esq., Dublin.

C. E., 18, female, complains of deafness with a running from her left ear since she had scarlatina two years ago. Hearing distance three inches. A muco-purulent discharge pours out of the meatus, at the bottom of which, upon a close inspection, two small globules of air can be perceived. On introducing the speculum those globules coalesced. Their presence is a positive and unfailing evidence of an opening in the membrana tympani; indeed I have never seen an instance where this symptom was not conclusive of the fact. I was lately called to see a gentleman said to be labouring under fever. He was extremely deaf, had from the commencement complained of violent pains in his ears; he had great heat of skin, furred tongue, loss of rest, and other febrile symptoms. Upon the fifth or sixth day a purulent discharge was observed to issue from both his ears. On examination I found several globules of air mixed with the discharge, and I at once stated to the attendant what subsequently proved to be correct, that the case was one of internal otitis, in which the tympanal membranes had given way, either by rupture, ulceration, or sloughing, and thereby gave exit to the pent-up pus in the tympanal cavities. Upon removing the discharge, an aperture was found in the inferior and anterior portion of the membrana tympani, opposite the entrance of the Eustachian tube.

In the case now under examination, we have another curious phenomenon, which has not, that I am aware of, been described by authors. By keeping the eye steadily fixed on this little bright globule, we perceive that it pulsates, and that its action is synchronous with that of the heart and arteries. I have remarked this symptom in similar cases for some years past, and I have frequently pointed it out to the class. It is not an invariable symptom, and the pulsation sometimes intermits. In order to see it in perfection there ought to be but a slight coating of discharge at the bottom of the meatus, and the globule from which the light is brilliantly reflected should be opposite the opening in the membrana tympani. In all the cases in which I have remarked it, the aperture was rather small, and situated in the posterior part of the membrane. I have never seen it where the opening was very large, or the membrane entirely destroyed. We can easily account for the existence of the globule itself; for so long as the Eustachian tube is free, any forced expiration, sneezing, coughing, or blowing the nose would produce it—a portion of the air thus driven through the tube becoming entangled in the thick viscid discharge through which it passes; but, unless the impulse is communicated to it by means of the general action of the arteries of the membrana tympani, it is difficult to account for the pulsation. By clearing away the discharge, we discover the truth of our diagnosis. The lining membrane of the lower portion of the meatus, as well as the external layer of the membrana tympani, is of a uniform pink colour, except at that part of the latter corresponding to the insertion of the malleus, which is whitish and prominent. Towards its upper and posterior part, we observe a circular aperture, about the size of a large pin's head, round the thickened margin of which the colour deepens in intensity. Upon desiring the patient to hold her nose, close her mouth, and then make a forced expiration, we hear a squealing noise, and we can see a quantity of thin fluid mixed with air pumped out through the aperture. When we have any doubt respecting the existence of perforation—for sometimes the aperture is so small or valvular that we are unable to detect it, especially if the sun is not shining on the part—we should always require the patient to perform this little experiment.

In cases of perforation the opening is, as I stated, generally opposite the aperture of the Eustachian tube, rather lead us to believe that it is caused by a burst or rupture of the a sudden jet of air striking against this thin portion of it w tension, rather than that it was produced by eith in cases li m inclined to think produced When a cornea is

from sloughing, hypopyum, or penetrating ulcer, we have an opportunity of observing the process from hour to hour. It is not so, however, in cases of inflammation of the drum; we have seldom an opportunity of examining the part until the mischief has occurred. When the disease is consequent upon, or happens during measles or scarlatina, &c., the ordinary medical attendant pays but little attention to the state of the ears, although the patient frequently complains of excruciating pains therein. He is satisfied with attending to the state of the fever and the eruption, telling the friends that the aural affection can be easily rectified after the patient's recovery. It must, however, be acknowledged, that in many instances the general symptoms of the disease are of such a threatening character, that both the physician and the friends are well satisfied if the patient escapes with life. Nevertheless, I cannot but feel, that an examination with the speculum should be made in all such cases, and means taken to relieve the aural disease by the application of a few leeches, &c. I have frequently saved eyes in patients labouring under small-pox, by employing the ordinary remedies applicable to pustules in the cornea.

In cases of inflammation of the middle ear and membrana tympani, we shall generally find upon examination, that the latter is one uniform sheet of redness, without any appearance of pointing, sloughing, or ulceration; and within a few hours after, the patient will tell us that he is relieved of his pain by something having suddenly burst into his ear, and then, upon inspection, we find a circular opening in the membrana tympani. I have seen an abscess in the layers of the membrane, and have elsewhere recorded a case of it, but that was a disease not of the cavity of the tympanum, but the drum-head alone.

I have touched the aperture in the membrana tympani with solid nitrate of silver, applied on the end of a flexible porte-caustic, which accurately covers the opening; and you perceive that it has caused a white ring round its edge. By this means I have frequently succeeded in closing holes in the membrana tympani of a much larger size. I suppose by inducing a process of thickening and contraction; but it requires time, and among the lower orders you seldom get the patients to attend sufficiently regularly to effect a cure. The girl was directed to have the ear gently syringed with warm water once a day, and afterwards to fill up the meatus with a lotion of zinc and alum, which is to be allowed to remain there for some minutes. The right ear is natural, and there is no tinnitus on either side. The moist-cotton remedy is unnecessary and inapplicable in cases of such minute perforation as this.

In perforation of the membrana tympani, particularly if the aperture is large, the patient generally complains of some of the water getting into the throat in syringing.

Perforation may occur without inflammation in bathing or diving, or even from the act of sneezing or violently blowing the nose; and I remember seeing a gentleman, some years ago, in whom it was caused by thrusting his finger into his ear to get rid of the water which had got into the meatus while in a warm bath.—*Med. Times and Gazette, March* 27, 1852, *p.* 314.

130.—*On the Treatment of Deafness by Glycerine.* By THOMAS WAKLEY, Esq., Surgeon to the Royal Free Hospital.—[Mr. Wakley brings forward further proof of the efficacy of this remedy in certain forms of deafness. He says:]

The class of cases to which I would draw attention in this report, are those of *cuticular* or *epithelial thickening* of the meatus, either *partial*, affecting the membrane of the tympanum, or *complete*, being continued over the entire auditory cul-de-sac. There is a greater or less degree of deafness corresponding with the amount of thickening; cessation of the secretion of cerumen; frequently tinnitus, or a "singing and hissing sensation" in the ears, and tickling irritation of the meatus. The causes are, constitutional predisposition, advanced age, chronic inflammation, long-continued discharge following eruptive fevers and the applications of escharotics and irritants. Amongst the latter, I would mention oily preparations, the *globules of which* adhere to the sides of the meatus or membrana tympani, and *become rancid, thus* producing a very frequent cause of inflammation. Upon exami-

nation of the affected ear, we find the meatus shining and inelastic, of a pearly whiteness, the membrana tympani either clouded or streaked, sometimes having small elevations upon it. The meatus is quite dry, the ceruminous glands being choked up by the epithelial growth.

The mode of application of the glycerine, when treating this state of the ear, is as follows:—The meatus is well cleansed with tepid water and then dried by means of the forceps and cotton, as described in 'The Lancet,' vol. i. 1849, p. 664. Glycerine is now poured into the meatus, and a plug of gutta percha, softened in boiling water, made to fit the external opening; this takes the exact form of the ear, becomes hard, and effectually prevents either the entrance of atmospheric air or the exit of the glycerine. The ear should be examined daily, and the same process repeated. The lining membrane can be examined with a blunt silver probe, passed gently through the speculum auris, to ascertain the effect of the glycerine upon the cuticular thickening. The meatus will gradually lose its shining pearly appearance, and softened pieces will fall off, and can be removed either by the forceps or gentle syringing. The practitioner should never attempt to tear them away, but allow them to come away by the means just stated. The treatment occupies ordinarily from two to four weeks, and is generally without any pain or inconvenience of any kind to the patient, and the results, in some cases, have been very gratifying. In the after treatment the patients are directed to moisten the auditory canal at least once a week with glycerine, applied by means of a camel-hair brush; this will generally prevent a recurrence of the cuticular thickening.

The *modus operandi* is simple enough—the glycerine being kept continually in contact with the part, acts mechanically, either absorbing or penetrating the epithelial coating, and separating the individual particles.

With respect to the permanence of the relief—some cases always require the presence of glycerine as the best known substitute for the natural secretion of the aural membrane. The frequent introduction of the glycerine tends to restore the external meatus to a healthy condition, and fit it for the proper transmission of sound.—*Lancet, May* 15, 1852, *p.* 467.

[Mr. Wakley warns us against procuring impure glycerine which he states to be frequently sold.]

DISEASES OF THE SKIN.

131.—ON ERYSIPELAS PHLEGMONODES.

By G. J. Guthrie, Esq., F.R.S., &c.

[Mr. Guthrie remarks that until the battle of Salamanca, in 1812, it was the custom of surgeons to stand quietly by and watch the progress of this peculiar form of inflammation, until life was either destroyed by the extensive mortification which ensued, or by the indomitable fever and affection of the brain which accompanied it.]

Accident (Mr. Guthrie proceeds) led me after that battle to perceive that the only effective means of relief in severe cases lay, in free incisions made into the part at an early period, according to the extent of mischief which had supervened; which mode of practice has been invariably inculcated in my lectures, from the year 1817.

The first case of the kind I had the opportunity of treating *publicly* and successfully in London, on the principles I had recommended, occurred in Westminster Hospital, of which I was one of the surgeons, in the year 1823. The example thus set was followed in St. Bartholomew's Hospital in 1825, and this practice has now become an admitted principle in surgery, of the first i —*Lancet, Jan.* 31, 1852, *p.* 11 .

132.—*On Urticaria.* ACLAGAN.—In a c
the urine was of low spe (1010), and was
much deficient in urea a hicum was
cess. The urine before as of

and left no deposit on standing. It was analysed according to Becquerel's method, and found to contain:—

Urea, ...	6·91
Uric acid, ...	0·05
Inorganic salts, ...	12·03
Organic matter and water, ...	981·01
	1000·00

Colchicum was then administered, and a fortnight after the urine was again examined. Density 1029·9. It was found to contain:—

Urea, ...	20·36
Uric acid, ...	0·50
Inorganic salts, ...	12·72
Organic matter and water, ...	966·42
	1000·00

Here then, it will be seen, that the physiological action of colchicum was well marked. The *urea* was more than triple in its amount, and raised above the normal standard. The increase of *uric acid* was in a tenfold ratio, whilst the other organic constituents and water suffered a corresponding diminution, the inorganic salts remaining nearly as before.—*Monthly Journal of Med. Science, Jan.* 1852, *p.* 27.

133.—*On Prurigo.* By Dr. J. M. MACLAGAN.—Dr. Elliotson gives the case of a man, labouring under this disease in its most inveterate form, to whom half a drachm of vinum colchici was administered thrice daily. This the patient took for three weeks; at the end of which time he was completely cured.

Colchicum would thus seem to answer well in some cases of skin diseases where the urine is of low specific gravity.—*Monthly Journal of Med. Science, Jan.* 1852, *p.* 27.

134.—*Case of Lepra.* By Dr. R. B. TODD, F.R.S.—[This was the case noticed in the preceding volume of the "Retrospect," as having been treated with wood tar. Dr. Todd reports the progress of the case.]

Wood tar, or Archangel, is the proper preparation for these cases: mineral tar was given by mistake in this case, and caused great irritation of the bowels. A convenient mode of administering the tar internally is in capsules, each containing fifteen minims, and he takes one three times a day; the tar is also applied externally over the patches by means of a brush, and the patient is kept constantly in tar sheets, that he may receive all the benefit possible from this plan. From time to time he has a bath to wash off the tar, in order to see the progress he is making. If you administer the tar in capsules, take care that the materials of which they are made are easily soluble, else they will pass through the bowels, tar and all, untouched. The first capsules that were given to this man were made of animal membrane; they did not dissolve, and he found that they came away in the stools. On changing them for capsules of gelatine, they ceased to appear in the stools, and the patient was conscious of their being dissolved in the stomach by the eructations tasting of tar. You may now observe in Cope that the spots are beginning to heal, and you should notice the mode of healing, which is characteristic of lepra, commencing in the centre, and spreading outwards to the circumference. Many of the patches have quite disappeared, and there is every prospect of the patient getting quite well, for a time at least; but the difficulty is, to prevent the recurrence of the eruption, and to effect a radical cure.—*Med. Gazette, Dec.* 19, 1851, *p.* 1048.

135.—*Sesquicarbonate of Ammonia in Lepra and Psoriasis.*—M. CAZENAVE, so well known as a very successful dermatologist, has just published experiments *tending to show that* sesquicarbonate of ammonia may advantageously be used as

a succedaneum of arsenical preparations, in lepra and psoriasis. The salt is mixed in the following proportions:—Half a drachm of sesquicarbonate of ammonia; diaphoretic syrup, seven ounces; take from one to three tablespoonfuls per diem. The physiological effects are very slight, but in the space of about a week the scales begin to fall off; those which succeed are thinner, the patches which give them support gradually fall in, the redness fades after a longer or shorter time, and a lasting cure generally ensues. If diarrhœa, lassitude, cephalalgia, quick pulse, and rapid alternations of heat and cold, were to occur, as was the case with two or three patients, the remedy should be suspended.—*Lancet, Jan.* 3, 1852, *p.* 12.

136.—*Itch Cured in Two Hours.*—Dr. Bazin, physician of the Hôpital Saint-Louis of Paris, introduced not long ago a notable improvement in the treatment of the itch, since he succeeded in curing the disease in *two days* by general frictions with the sulphur ointment. Dr. HARDY, who has succeeded Dr. Bazin in the Scabies wards of the same hospital, has, however, considerably curtailed this already short time; he cures his patients in *two hours.* The method is described as follows:—Patients are no longer admitted *into* the house for the treatment of the itch, as two hours suffice to render contagion impossible and the recovery almost certain. The patient is put into a warm bath, and rubbed for an hour with yellow soap; he then passes into a clean bath where he continues to cleanse his skin for another hour. After leaving this bath he is taken to a particular room fitted for the purpose, and, with the aid of one of his fellow-sufferers, he is rubbed all over for half an hour with the following ointment:—Axunge eight parts, flowers of sulphur two parts, carbonate of potash one part. After this friction, the patient is examined and sent away cured, though sometimes pretty numerous vesicles on the hands and elsewhere remain unaltered. Dr. Hardy states that out of one hundred cases he has hardly had two or three relapses. The number of itch patients has considerably diminished, as none are now turned away for want of room; and the disease has thus spread with much less rapidity.—*Lancet, Dec.* 20, 1851, *p.* 583.

137.—*On Eruptive Diseases of the Scalp.* By CHARLES POOLE, Esq., Enfield.—[Mr. Poole observes:]

For the suggestion of the following remarks and plan of treatment I am indebted to Dr. Neligan, of Dublin, whose simple and practically efficient division of these diseases into inflammatory and non-inflammatory I have followed, and this I believe to be the true basis on which to build a rational plan of treatment.

Inflammatory.—Herpes capitis; vesicular; contagious. Eczema capitis; vesicular; non-contagious. Impetigo capitis; pustular; non-contagious. Pityriasis capitis; scaly; non-contagious.

Non-inflammatory.—Porrigo capitis; vegetable growth; contagious.

Of course there are other eruptions found on the scalp, but they are in connexion with those generally situated on the other parts of the body, and therefore in this situation require no particular description.

As regards prognosis, this depends more on the length of time any given eruption has lasted, than on any particular kind. When seen early, and properly treated, they are cured in from a fortnight to three weeks, sometimes much sooner; but some old chronic cases require from one to three months. They seem to be curable generally in the following order:—1st, impetigo; 2nd, pityriasis; 3rd, moist eczema; 4th, lupus; 5th, dry eczema.

The treatment is based on the fact of these affections being both inflammatory and constitutional. That they are inflammatory is sufficiently evident; that they are constitutional is almost proved, from the advantage derived from alterative medicines. The absence of this division has formed, I fancy, the chief stumbling-block. Stimulant applications, usually applied in chronic cases, frequently fail, and in recent ones are obviously injurious. As a general rule, in these diseases, the hair must be cut close with a pair of scissors, preceding treatment; shaving, I believe, from the attendant irritati

jurious; and the head should be covered with an oil-skin cap. The local plan of treatment consists of ointments and lotions of the carbonates of soda and potash, in greater or less strength. The carbonate of potash, being the stronger preparation, is more adapted for the chronic forms, and when the attendant inflammation is slight. The quantities of the carbonates, used as unguents, vary from twenty grains to one drachm, to one ounce of prepared lard—as used in lotions, from half a drachm to one and a half, in a pint of rose or distilled water. The ointment is to be applied three times a day, smeared over the eruption, and washed off each morning with the corresponding lotion. In cases where crusts or scales are found, a linseed-meal poultice applied for twelve hours, and the ointment for a similar period, render them easily removable by washing gently with the lotion. Sometimes, the unguents disagree; in that case, the lotion must be substituted, but used four or five times daily. In the chronic forms, when stimulants are necessary, they are best treated with an ointment consisting of from half a drachm to one drachm of the citrine ointment to one ounce of prepared lard; this applied at bed-time only, and the lotion during the day. The alterative medicine Dr. Neligan uses is the yellow iodide of mercury (Proto-iodide P.L.), in combination with mercury with chalk, and aromatic powder. To a child six years old he gives half a grain of the iodide, two grains of mercury with chalk, and two grains of aromatic powder, every second morning; for an older child the same every morning; to a younger child every third or fourth morning; for infants he omits the iodide; but I have found mercury with chalk, aromatic powder, and sesquicarbonate of soda, given more frequently, answer equally as well. During treatment, the child should be kept strictly on milk diet. This plan of treatment I have seen employed, and have used it myself for more than five years with the most unswerving success. I have not seen a single case resist this plan.—*Lancet, April* 17, 1852, *p.* 377.

138.—*Starch in Skin Diseases.*—M. Cazenave is now substituting powdered starch for poultices, especially in pruriginous affections, either simply dusted over the part, or applied after the latter has been well cleansed by an alkaline solution and thoroughly dried. The starch may be used simply, or mixed with oxide of zinc, camphor, &c. M. Cazenave treats acute eczema, acne rosacea, impetigo, and herpes, by dusting the affected regions, night and morning, with the following powder—White oxide of zinc, two drachms; powdered starch, four ounces. Very good results have been obtained in prurigo of the genital organs, the groin, or the axilla, with the following powder:—White oxide of zinc, two drachms; camphor, half a drachm; powdered starch, four ounces.—*Annales des Maladies de la Peau.* —*Lancet, May* 8, 1852, *p.* 442.

139.—*Treatment of Ulcers by the Exclusion of Atmospheric Air.* Under the care of B. Holt, Esq., at Westminster Hospital.—[Although all the measures so recently advocated to promote the healing of ulcers have been more or less attended by success, yet it would appear that the exclusion of atmospheric air and very rare dressing are conducive to the most satisfactory results. In the first case Mr. Holt mentions the patient is a carpenter, who had previously been in Middlesex Hospital, with an ulcer the size of the hand, and one-eighth of an inch deep. After leaving that Hospital, in a few days, the ulcer had attained its original size, and he then presented himself for admission under Mr. Holt.]

When the inflammation had been subdued the ulcer was dressed in the following way:

From a piece of adhesive plaster, somewhat larger than the sore, a portion, just the size of the latter, is cut out; the plaster is then applied to the part, and painted with collodion. Oiled silk is now placed over the ulcer, and made to adhere to the plaster, by means of the collodion, by which process the air is completely excluded from the ulcerated surface. The whole is then secured by strips of adhesive plaster placed crosswise, and by a roller running from the toes to above the knee.

This dressing used to be left undisturbed for a week or ten days, and by being taken off with care did not give the least pain. The patient remained in the meanwhile mostly in bed, and had good diet. On the 23rd of March, about nine weeks after admission, the sore was completely healed, but by walking up and down stairs it re-opened to the size of a fourpenny-piece. The same kind of dressing was continued, and in a fortnight the patient was discharged, the ulcer being quite cicatrized.

Case II. Richard C——, a labourer, aged forty, was admitted March 12, 1852, under the care of Mr. Holt. The patient had been run over the loins by a cart, and had escaped without much injury. Whilst under treatment he complained of an old ulcer on his right leg, about the size of the palm of the hand. Poultices were applied for a few days to subdue inflammation, and the same kind of dressing as described above, was employed. This patient, however, was allowed to leave his bed, and walk about the ward. In the space of three weeks the sore was healed, and the man has been discharged.

Mr. Holt has treated numerous patients in this manner with perfect success. We, however, noticed one case of very chronic ulcer (of twenty years' standing), under the care of Mr. Phillips, which from some peculiarity of constitution did not benefit by the treatment. Ulcers of the lower extremities are so common, that we gladly acquaint our readers with the above-mentioned line of practice, as it has in many cases been found to act very beneficially.—*Lancet, April* 24, 1852, *p.* 399.

SYPHILITIC DISEASES.

140.—ON THE EMPLOYMENT OF MERCURY IN ORDINARY CASES OF SYPHILIS.

By G. Borlase Childs, Esq., Surgeon-in-Chief to the City Police Force, &c.

During the ulcerative stage of an ordinary chancre, I believe there are few practitioners who would place their patients under the influence of mercury; such a practice would be fraught with danger, and, to say the least of it, the progress of the case would be slow, and much more protracted than it ought to be,—circumstances both embarrassing to the surgeon and unsatisfactory to the patient. If, therefore, an opportunity occurs of seeing the patient during the earlier stage of chancre, the right course will be to destroy the pustule by a liberal use of the nitrate of silver, or, what is far better, with a finely pointed stick of caustic potash; less irritation will be found from the latter application than the former; the sore should then be treated with simple dressing, or with a weak solution of chloride of soda and opium, over which a layer of finely-carded cotton should be placed, in order to obviate the possibility of friction. This treatment should be pursued until the regenerative process commences, when mercury may be resorted to with marked benefit. These sores are not unfrequently contracted at a time when persons, having drunk more freely than prudence suggests, a sort of artificial incentive is acquired for sexual indulgence. With these persons the system is in an irritable, inflammatory state, and the sores so contracted necessarily assume an irritable, inflammatory type. Under such a state of things it would not only be unwise, but dangerous, to place a patient under a course of mercury,—we must wait until the system has been cooled and quieted by mild aperients and opiates, the sore freely cauterised, and subsequently dressed with the solution referred to. It will sometimes be necessary even to abstract blood from the sore itself by the application of a leech, care being taken that an additional chancre is not created from the l[illegible]hite itself.

In spite of all our efforts, from the onset some sores [illegible] resist every attempt made to heal them, and evince a disposition to [illegible] and sloughing, to which we apply the term phagedæna. Some [illegible] superficial, others of [illegible] character; many are amenable to [illegible] there are [illegible] in which this remedy totall[illegible] which lar[illegible] and opium can alone b[illegible] that the [illegible] ed, at first sight, from form a com[illegible] the plan of treatmen[illegible]

curial or otherwise; but, granting that the sore had a venereal origin, and that the patient had not been previously submitted to a mercurial course, with certain restrictions, he would not err in adopting it. In short, as a general principle, mild doses of mercury, in the form of blue pill and Dover's powder, may safely be tried in all ordinary cases of venereal chancre, without any ill result.

With regard to the second question, viz., the influence of mercury over venereal sores, there can be little doubt but that a great many of these sores heal without the aid of mercury, and there can be still less doubt but that a modified mercurial treatment greatly facilitates the healing process, and materially diminishes the chances of secondary symptoms. Were I asked, therefore, to define the character of a sore in which the simple treatment should on no account be risked, I should say, in a few words, all sores that have a tendency to induration and an indisposition to heal. And here I may also remark, that if we are disposed to be exclusive in the use of this remedy, we must learn to distinguish those sores which derive their induration from simple irritation, and those which acquire it from the application of a specific virus, as all the characteristic appearances of a true Hunterian chancre are not unfrequently simulated in a similar sore by a too liberal use of lunar caustic and other irritating applications.

And, now, as to the power of mercury in preventing constitutional syphilis,—a question of grave importance to the patient, and one of undoubted interest to the surgeon,—a question which, I am free to confess, has not been satisfactorily responded to in my own mind, either by my own personal experience or by the collated opinions of others;—to place implicit confidence in the anti-syphilitic powers of mercury, or, during the primary chancre, to guarantee a patient's safety in this respect, would be but a bold and adventurous speculation, and one unbecoming a follower of legitimate practice. Certainly much can be done to lessen the chances of such a result, by judicious management in the early stage of chancre, such as free cauterization, rest, and a proper prophylactic treatment. It has been asserted by a celebrated writer on this disease, that all persons are not susceptible of a general infection. In this opinion I concur, and it must have been observed by those who have had much experience in this department of practice, that persons who, from neglect or otherwise, have not touched a grain of mercury, yet with a sore possessing all the characteristics of a true Hunterian chancre, are never attacked by consecutive affections; while, on the other hand, those who from the commencement have been submitted to a mercurial course, have suffered most severely from them. In such an uncertain state of things, the wisest course is to avoid extremes by steadily administering moderate doses of blue pill, combined with Dover's powder, until the sore has healed, and no induration remains. By this plan, although we cannot offer our patient a sure guarantee against constitutional infection, yet there is one thing at least we shall have the satisfaction of knowing, that should consecutive affections appear, in the majority of cases they will be so modified and mild in their character, that little or no uneasiness need be felt respecting the results.—*Med. Times and Gazette, Jan.* 17, 1852, *p.* 62.

141.—*A Substitute for Mercury in Syphilitic Diseases.* By M. E. Robin.—[M. Robin stated before the Academy of Sciences of Paris, that mercury merely combined with the syphilitic virus and changed it into a new compound, and that there were many substances which form analogous compounds with organized matter, which substances have, like mercury, anti-syphilitic virtues, belonging to the antiseptic division of remedies which act by combining with the noxious principles.]

M. Robin induced Dr. Vicenti, of Paris, to try a few experiments with the bichromate of potash to combat syphilis; the salt was employed in three cases with much success, and of these one was marked by very severe secondary symptoms. Fifteen grains of the bichromate were divided into eighty pills, with extract of gentian. One of these was taken night and morning. They agreed pretty well *with the stomach*, though some opium was necessary to prevent nausea and vomiting. *The patient took* 240 pills in the space of about three months, and was then

quite well of a very intense attack of iritis, accompanied by other syphilitic symptoms, which had almost blinded him. The patient had had an indurated chancre, and had never taken any mercury.—*Lancet, Dec.* 27, 1851, *p.* 603.

142.—ON OPENING BUBOES BY MULTIPLE PUNCTURES.

By M. Vidal.

M. Vidal strongly recommends that venereal buboes should not be allowed to open of themselves; for when they are left to nature, the skin becomes detached and thinned, a great loss of substance ensues, a tedious recovery takes place, and an unsightly deformity is left. Opening by caustic, too, leaves disfiguring scars; and the same inconvenience results from large incisions, and cutting away, by the bistoury, portions of skin that are too much changed to unite. Still the bistoury is much more easily managed than is the caustic, and M. Vidal much prefers removing by it a portion of half dead, detached skin, or a gland which is an obstacle to reparation, to attacking such parts by caustic. By its aid the cicatrix may be rendered more regular, and cause less deformity. But cases requiring large incisions and excision are rare, especially if the bubo be early treated, and punctures are made as soon as matter is formed.

The bubo should be shaved, and any remains of plaster, &c., removed. A straight, narrow bistoury, or even a lancet, may be employed. If the abscess is recent, and suppuration not extensive, *one* puncture at the fluctuating point may be employed, as a single gland may be then inflamed, and we thus discharge the pus from it. The other glands are only engorged; but if they afterwards suppurate, they must be treated in like manner—so that in this way we may have to open three or four of them in succession, especially in scrofulous subjects. When the abscess is intra-glandular, its deeper seat renders it more difficult of recognition; and it is far better opened by a straight bistoury, which also acts as an exploratory instrument, than by caustic. If the collection be both extensive and superficial, several simultaneous punctures are required. These must not be practised in the thin skin of the fluctuating centre, but at the circumference. The bistoury is passed in obliquely, and guided subcutaneously towards the centre, arriving thus at the pus by a circuitous route. In this way, we divide the skin only where it is adherent, intact, and possessed of its vitality; while if we penetrated the denuded, half-dead, and thinned skin of the centre, there would be danger of the apertures enlarging by mortification, and approximating to each other so as to constitute a large breach of surface, giving admission to the air, and being followed by all the disadvantages of large openings made by the knife or caustic. The bubo must not be pressed for two days after making the punctures, and it will then gradually discharge itself, the space being filled up in proportion as the pus is evacuated, and the walls of the abscess are retracted. The cure is rapid, and after its completion not a trace of the disease is left—the cicatrices which remain not showing more than leech bites, and like them eventually disappearing. Sometimes the little oblique tracts made by these punctures become obliterated before the pus is completely evacuated; but as it is rare for them all to do so, we may, by gently pressing the tumour once a day, discharge the pus by such of them as remain open—this compression being uncalled for during the first day or two, when the tracts are free, and the pus abundant. Occasionally all the tracts show a great tendency to close up, and it is preferable to allow them to do so, rather than apply tents to keep them open—making one or two new punctures if required, which is not always the case, as the remaining pus is sometimes resorbed.—*Bull. de Thérap.* vol. xli. pp. 204—212.—*Brit. and For. Med. Chir. Review, Jan.* 1852, *p.* 267.

143.—*On the Treatment of Gonorrhœa.* By Dr. P. Niddrie.—statement made by a member of the Medical Society on the discus ject, "that time alone would cure the disease," Dr. Niddrie ob

treating gonorrhœa, by which the patient is generally cured in three days, and almost always within a week. He says]

During the first day, a saline purge, such as a Seidlitz powder, with half an ounce of sulphate of magnesia, is to be given: recumbent rest enjoined; weak linseed tea, with a little nitrate or bitartrate of potash, used as common drink; and ordinarily pure cold water used as an injection twice in every half-hour. During the second day, the same drink to be used, and quiet observed, but a solution of sulphate of zinc, two grains to the ounce, is to be substituted for the cold water, and used twice every half-hour during the day. On the third day the irritation and discharge will probably have gone, and it will not be necessary to enforce rest so strictly, but the drink and injection must be used as on the preceding days. These remedies are commonly in use, but their efficacy depends on the mode in which they are applied; and if this method is strictly followed, few unsuccessful cases will occur.

Doubtless there are cases protracted for weeks or even months, but such patients fancy it is too irksome to lie down all day, and it is too much bother to use the injections so often, and they expect to be cured without trouble or restraint. Indeed, there is always difficulty to get patients to use the injection so frequently and perseveringly as is necessary, but on this the success of the treatment mainly depends. It occasionally happens, that on the second day the swelling of the urethral membrane, its irritation, and its discharge, are not sufficiently allayed, and it is necessary to continue the cold-water injection till the third day. More frequently it is necessary to use the sulphate-of-zinc solution longer than two days, for it must be used at least a day after the discharge has stopped; but it will rarely happen that the whole period of treatment extends to a week.

Whether or not gonorrhœa is a specific disease, there unquestionably exist in it redness, swelling, heat, and pain—that is, inflammation, terminating in suppuration: and the antiphlogistic means indicated seem to me a rational treatment of such a state of parts. But if the inflammatory action only be subdued, disordered action continues in the form of gleet, and it becomes necessary to change the action of the mucous membrane by a slightly stimulating injection of sulphate of zinc.—*Lancet, April* 10, 1852, *p.* 357.

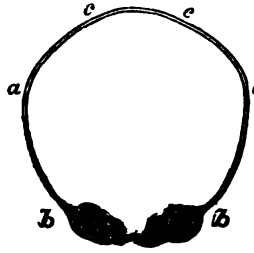
MIDWIFERY,

AND THE DISEASES OF WOMEN.

144.—TURNING AS A SUBSTITUTE FOR CRANIOTOMY IN LABOUR DELAYED BY OBSTRUCTION AT THE BRIM OF THE PELVIS.

By Professor Simpson, Edinburgh.

[Special cases of difficult labour show that in particular forms of difficult labour and deformed pelvis, it is easier to deliver by bringing down the feet than by allowing the head to form the presenting part. For if we make a transverse section of the fœtal skull, we find its bimastoid diameter, *b b*, is considerably less than its biparietal diameter, *a a*. The difference in these two diameters in the child at birth generally amounts to from one-half to three-quarters of an inch. Besides, at its bimastoid portion or base, the bones are so strong that it is quite unyielding, while at the parietal surfaces the bones are thin and elastic, and its sutures are so imperfectly united, that the head may be laterally compressed, or even depressed and indented, without necessarily destroying the life of the child. Dr. Simpson goes on to say:]



Since writing upon this subject at considerable length a few years back (see 'Provincial Medical and Surgical Journal' for 1847–48, see also 'Retrospect,' Vol. XVIII. p. 348), I have repeatedly had occasion to turn the child in difficult cases, where the head was not far entered into the brim, and where the long forceps failed, or were contra-indicated; and in which the alternative of craniotomy seemed the only other measure that could be adopted. A number of my professional brethren have reported to me the success with which they have also followed this practice. In the way of illustration, I adduce the three following cases which have occurred in my own practice and in that of my friends, Dr. Weir and Dr. Peddie, within the last few weeks. I have the pleasure of detailing the cases recently met with by Dr. Weir and Dr. Peddie in their own words.

Case I. On the evening of Jan. 11th, I was asked to see a case of lingering labour, under the charge of Mr. Keeling and Dr. Cooper. The patient, æt. 26, and pregnant for the first time, had been in labour for about forty-eight hours. The first stage of labour had been terminated about thirteen hours before I saw her, and the head had remained at the brim of the pelvis, without advancing in any degree further down, for upwards of ten hours. On examining, I found the vagina fully relaxed, but its mucous membrane was becoming heated and œdematous, in consequence, in all probability, of the lengthened obstruction at the brim above. The infant's head was elongated down into the cavity of the pelvis; but the broad part of the head had not passed the brim. The promontory of the sacrum was so easily reached by the finger, as at once to give the idea, that it projected forwards to an extent greater than natural. The direction of the sagittal suture, showed that the head lay more transversely than in the usual normal presentation. The face of the infant was directed to the right sacro-iliac synchondrosis, or rather to the right ilium. The patient was put fully under the influence of chloroform, and the long forceps were easily applied. The blades of the instrument were, as we found after the birth of the child, applied, as usual, obliquely over the head, and did not offer to slip in any degree under the traction that was applied. But no amount of traction that I thought it justifiable to employ could move the head downwards: and Mr. Drummond failed also in making any impression vancement. the cranium when I gave him the instrument to use. veral the direction of the traction, and the position of the success; and, at last, withdrew the forceps. Concl

be some obstruction to the advancement and passage of the child, from some malposition of the arm about the neck, or from some mal-formation, I introduced the hand by the side of the child's head for the purpose of ascertaining these circumstances, but found nothing that appeared to me capable of explaining the delay, and the impossibility of advancing the head with the forceps, except it were some oblique position of the head, relatively to the neck or trunk of the infant. A few years ago, I would in such a case have, perhaps, deemed the operation of craniotomy the only remaining resource. But the stethoscope showed that the child was still alive and well. And, under these circumstances, I resolved to attempt to extract it by the operation of turning. The patient was deeply anæsthetised, in order to relax the uterus as much as possible; and, at a time when all uterine contraction seemed absent, I passed up my hand and brought down one of the lower extremities of the child. When doing so, a large loop of the umbilical cord fell down into the vagina. I now found an obstruction to the complete version of the infant, which I had met with previously in other cases of turning, when the head was the original presenting part. For, though one foot was down at the orifice of the vagina, the version of the fœtus upon its own axis had not been complete, and the head was still at, or near, the brim. Consequently, while Mr. Drummond held and retained the extruded foot, I passed up my right hand to the head to push it upwards, so as to complete the version,—a part of the operation, which in this, as in other cases, is always much aided by the manipulation of the left hand upon the abdomen externally. Subsequently the trunk and arms were easily extracted, and the traction requisite to make the head pass the brim, was much less than I had seen in several similar cases. The child, after birth, had the heart still pulsating; and was readily revived by repeatedly plunging its body alternately from a warm into a cold bath. Yesterday (21st January) I saw both the infant (which was, perhaps, rather above the usual size) and the mother, quite well.

Case II. On the evening of the 2nd Dec., 1851, I was requested (Dr. Weir writes me) by my pupil, Dr. Bone, to give an opinion upon a case of protracted labour. The patient had been eighteen hours in labour of her fifth child. She had been, in previous labours, the subject of tedious and difficult labour, except in one confinement, when the child was born below the usual size. Her face was flushed, eyes suffused, skin hot, pulse quick, and restlessness great. The os uteri was completely dilated, and the liquor amnii discharged. The head was still at the brim of the pelvis, and although the pains had been for many hours quick, strong, and expulsive, a very small portion of it penetrated the pelvic cavity; and so much as did receded above the brim upon the cessation of the uterine contractions. Under these circumstances, I determined to deliver her immediately, and as it appeared a favourable case for turning, decided upon doing so in preference to using the long forceps. The chloroform was administered till deep snoring was produced, and the hand introduced in the usual manner. So completely were the uterine efforts suspended, that I grasped both the feet before the uterus contracted in the slightest degree upon my hand. No difficulty was experienced in extracting the child till the head reached the brim, when considerable force was required to draw it through; but not so much as to prevent its being born alive. The mother did well, and in a few days was attending to her household duties. The child presented, after birth, a deep depression of the cranium, on the anterior part of the left parietal bone, immediately above the ear,—the result, no doubt, of compression against the promontory of the sacrum. But this has not affected the health of the child, which now (20th Jan.) is otherwise thriving and well.

Case III. On the 30th Nov., 1851, I was called (writes Dr. Peddie) to Mrs. ——, aged 50, in her sixth confinement.

At her first confinement, Nov. 10, 1843, in consequence of contraction at the brim of the pelvis, principally on account of an exostosis projecting from the promontory of the sacrum, and to some extent also from an under average size of the pelvis, she was delivered by embryulsio. This extreme measure was not resorted to until forcible natural pains had existed for many hours, without effecting an entrance for the head at the brim of the pelvis; until the long forceps had been applied, first by myself, and then by Dr. Simpson, without obtaining any advance of the head; and until the fœtal circulation had been ascertained to have ceased.

After the perforation was employed, no small difficulty was experienced in dragging the child through the pelvis.

At her second confinement, 26th Sept., 1844, after allowing four hours to elapse from the time when the os uteri was fully dilated without the smallest descent of the head, I succeeded, with the long forceps and powerful traction, in delivering the child safely. This fortunate termination was the more satisfactory, that the induction of premature labour had been considered and decided against some months previously.

At her third confinement, 20th April, 1846, the labour went on naturally, and the child was expelled without artificial assistance. The explanation of this fortunate event appeared to be, that the child was a female one, and considerably under the average size, the head, more particularly, being small.

At her fourth confinement, 28th Oct., 1847, the labour resembled Mrs. ——'s first, the first stage being short, while the second had existed for many hours before I saw her. The pains were extremely forcible, conveying the impression of danger from rupture of the uterus. After trying two different pairs of long forceps, I sent for Dr. Simpson, who furnished me with a third pair—which, though easily applied, and powerful, did not enable me to bring forward the head in the least degree. Dr. Simpson then proposed turning in preference to perforation—the more especially as the child was ascertained to be still alive; and this he accomplished readily, the patient being very deeply under chloroform—and delivered her after the employment of very powerful traction. The child was at first as if still-born, but was brought about after the continued use of the usual means.

At her fifth confinement, 11th Dec., 1848, the presentation was a footling one—as if nature was indicating the right mode of procedure; and, accordingly, when the first stage was completed, I seized hold of the other foot, brought it down, and delivered the child safely, but not without the employment of very considerable traction in bringing the head through the pelvis.

At her sixth and last confinement, 30th Nov., 1851, I saw Mrs. —— at 11 a. m. Pains had begun about twelve o'clock on the previous night, and had gone on regularly, but not severely, until within two hours from the time of my visit. I found the os completely dilated, the vagina filled with tensely distended membranes, and the head above the brim. On passing the hand to make a proper examination, I recognised the old exostosis, but decidedly larger, fully the size of a small walnut, projecting from the sacrum immediately within the right sacro-iliac symphisis. I found also, that the head was lying with the occiput to the sacrum, pointing somewhat to its left iliac symphisis, and the forehead to the right side of the symphisis pubis. I was also satisfied, that the head was of large size, while the pelvis generally was small.

Considering previous experience in the case of this patient, and the remarkable success of turning in her fourth labour, when she could not have been delivered otherwise than by the life-sacrificing perforator; and considering that in the present labour the peculiar position of the head would render efforts by the long forceps—however well employed—useless, I resolved at once to turn. This I accomplished, and brought down the body very satisfactorily, while the patient was placed deeply under chloroform by Dr. Harper, of Leith, who kindly assisted me. As was to be expected, much exertion and pulling were necessary to drag the head through the pelvis, although the most favourable respective diameters were chosen. A towel slipt round the anterior part of the neck and chest, and crossed over the shoulders behind, gave steady purchase and enabled me more readily to complete the delivery. The child was a large boy, and at first apparently still-born, but in about twenty minutes, after unceasing attention by giving alternate plunges in hot and cold baths, as practised in the first and second labours, and by the use of artificial respiration, he was completely restored. He ultimately did well, and Mrs. —— made an excellent recovery. The child weighed 9 lbs. 5 ozs.

[Dr. Simpson adds:]

In conclusion, let me briefly recapitulate some of the principal advantages which, as it appears to me, the operation of turning has over the operation of craniotomy, in cases such as we have been considering in the present communication, viz.:—where the pelvis is somewhat too small, or the fœtal head somewhat too large, to

allow the infant to pass by the unaided efforts of nature, or even with the assistance of the long forceps, if that instrument is had recourse to.

1. It substitutes the delivery of the infant by the hand of the accoucheur, for its delivery by formidable steel instruments. And certainly the avoidance of instruments is, as a general principle, desirable when it is possible.

2. The transit of the cone-shaped head of the child, through a somewhat narrow brim, is facilitated by the narrow end of the cone (or bimastoid diameter of the head) being made to enter and engage first in the contracted brim; and the hold which we obtain of the extruded body of the child, enables us to employ so much extractive force at the engaged fœtal head, as to make the elastic sides of the upper and broader portion of the cone (or biparietal diameter of the cranium) to become compressed, and if necessary indented, between the sides of the contracted brim.

3. When the child is brought down footling, we have far more power than when the spherical arch of the cranium presents, of manually adapting and adjusting, when necessary, the shape of the head to the shape of the contracted brim; the rounded form of the cranium not affording us any sufficient hold and purchase for this purpose in cranial presentations.

4. The *lateral* and very *temporary* compression of the fœtal head, by the contracted sides of the pelvis, such as we can produce and effect on artificial turning and contraction, is less dangerous to the life of the child than its *oblique* or longitudinal compression with the long forceps, or by the *long* impaction of the head itself in the contracted brim.

5. In cases where the narrowness is greater, and such as to produce a depression or indentation in the elastic and flexible cranium of the child, still this transient depression, or indentation, is not necessarily destructive to life, as the perforation of the head in craniotomy is. Children often survive and recover, when born with the head much distorted and even indented See, for example, the child in Case 2.; and other similar instances recorded by Smellie, Denman, Velpeau, Jacquemier, Radford, &c.

6. On these accounts, the operation of turning affords a fair chance of life to the child; while craniotomy affords none. And even when the turning and extraction require some considerable time for their performance, the resulting temporary asphyxia of the child is not necessarily so deep and fatal, but that the infant may be revived by appropriate measures applied after birth. I can, for one, state that in these cases, and in instances of common footling and turning cases, I have repeatedly been astonished at the viability of the infant after traction had been applied to it, both so strong in degree and so long in duration as to leave apparently little hope of its survival: and I have heard other practitioners make the same remark as the result of their experience.

7. The operation of turning, under the circumstances we speak of, will, I believe, be found also to be more safe to the life of the mother than the operation of craniotomy. In every instance, the operation of craniotomy is necessarily fatal to the infant; but in a very large proportion also, this operation is fatal to the mother. The statistical results collected by Dr. Churchill and others, show that craniotomy is fatal to the mother in about 1 in every 5 cases in which it is performed; while turning does not generally prove fatal in above 1 in every 15 or 16 patients, even including complicated cases. Besides, it affords this great source of safety to the mother, that, *cæteris paribus*, delivery by turning can be, and is, as a general rule, adopted far earlier in the labour than delivery by craniotomy; and in proportion as it is practised earlier, so far also will it be practised with greater safety and greater success,—the maternal mortality attendant on parturition, whether natural or operative, increasing always in a ratio progressive with the increased duration of the labour.

Among the operative deliveries which occurred in the Dublin Hospital, when Dr. Collins was master of the Institution, the duration of the labours at the time of operating is stated in 125 cases. Among these 125 instrumental and operative deliveries, only 1 in every 17 of the mothers was lost, when the delivery was lished within twenty-four hours from the commencement of labour; 1 in f the mothers died when the delivery was delayed till from twenty-four *ight hours*; and nearly 1 in every 2 mothers perished when the delivery *ed till the* labour had gone on above forty-eight hours. Obstetricians

have often argued, that if, in cases of obstructed labour, the delivery is delayed for a sufficient length of time, the child will be ultimately destroyed by the uterine action and compression, and that thus craniotomy will be at last performed upon the dead infant,—the child being killed by an act of omission and not of commission. But even such very protracted delay is not always fatal to the infant, some continuing to survive when the labour is prolonged for sixty, or seventy, or more hours. And it is always to be remembered, that the delay itself, if dangerous to the life of the child, is also, as the above and other evidence shows, almost equally dangerous to the life of the mother. In such cases of long obstruction and delay, even after the head is perforated by craniotomy, much traction is often required to drag the shoulders through the contracted brim, and that at a time when the structures at the brim are so damaged by previous pressure, as to be little able to bear compression with safety. And I do think that we have most ample grounds for believing, that the *long* compression of the soft parts, such as occurs in very protracted labour, is more truly dangerous to the structure than a *short* compression of them, greater in amount, such as occurs in the operation of turning when early performed. (See, on this point, 'Provincial Medical and Surgical Journal,' for 1848, p. 534.)

In not a few cases, in which the operation of turning is resorted to in consequence of the complication which we have been considering, the practitioner must be prepared to meet with such resistance to the passage of the head through the brim as will require some adjustment and considerable physical exertion on his part, in order to overcome it. But if the head be so adjusted in the brim, that the shape of the one is, as much as possible, adapted to the shape of the other; if the chin be kept depressed towards the sternum; and if the traction applied be made in the proper axis of the brim itself, no small amount of extractive force may be used without compromising the safety of the mother or infant; the degree of traction which the structures of the infant's neck will in this way undergo, is much greater than one would *a priori* suppose. And as a very general rule, the elastic lateral walls of the cranium of the child will become compressed or indented, before any dangerous injury is inflicted upon the structures of the neck. But on this subject I most willingly substitute for any remarks of my own, two or three sentences from the work of an author,—always practical and always cautious,—Dr. Denman, who, in speaking of the occasional difficulty of extracting the head in common pelvic and turning cases when the brim is somewhat contracted, gives, among others, the following directions:—

"The force with which we endeavour to bring down the head of the child, must then be gradually increased, till we are convinced, that a greater degree is inconsistent with the safety of the child, or induces the hazard of separating the body from the head. Should the head descend in ever so small a degree, we must not act precipitately, and increase the force in order to finish the delivery suddenly; but we must proceed with circumspection, or we shall add to the danger which the child is already in, and run the risk of doing injury to the mother; though, when the head begins to advance, there is seldom much remaining difficulty, the cause usually existing at one particular part of the pelvis. It has been said, that children have sometimes been born alive, when the strongest efforts, and these continued for many hours, have been made to extract the head detained in this position. But I have not been so fortunate as to meet with any such instances, a short space of time having generally been sufficient to frustrate my hopes, and convince me that the child was dead. Though when the head has been detained a considerable time, a few cases have terminated more favourably than I could have expected, and I have been agreeably surprised with the discovery of some faint signs of life, which by the assiduous and careful use of the common means, have been improved, and the life of the child at length perfectly recovered. * * * When we have in vain exerted all the force which we think reasonable and proper, and which, in some cases, must be more than any circumstance would be thought to require, it will be expedient to rest, for the purpose of gaining all the advantage to be gained by the compression of the head. On this account, the mother will actually suffer no more inconvenience, than would have been purchased if the head had originally presented, and been locked in the pelvis. After waiting some time, we must renew our attempts to extract, and thus proceed, alternately resting and acting with effi-

cacy and resolution, and if the hold we may have of the body or extremities of the child does not suit, a silk handkerchief or other band may be passed round its neck, and this will be found a very handy and convenient instrument. It must (Dr. Denman adds) be a very great disproportion between the head of the child and the pelvis, which is able to withstand this method of proceeding, if we persevere in it with prudence and steadiness; because the integuments of the head will burst, or the bones be bent inwards in an extraordinary degree, or even broken. * * * (If it fail) it then only remains that we should lessen the head of the child; and the operation may be as easily performed in this, as in the natural presentation of the head. When (he continues) the perforation is made, and the brain evacuated, the head may be readily extracted either by pulling by the body of the child, or by inserting a crotchet in the opening made by the operator as in other cases. But it will be scarcely believed how seldom this operation is necessary under these circumstances, if we have not been in a hurry, but have acted with prudence. Nor (concludes Dr. Denman) have ever I known any ill consequences follow the compression which the soft parts undergo, between the head of the child, and the sides of the pelvis, if proper attention were afterwards paid to the state of the bladder and rectum.—*Monthly Journ. of Medical Science, Feb.* 1852, *p.* 135.

[In reply to this practice advocated by Dr. Simpson, Dr. RAMSBOTHAM raises the following objections. He says:]

"It is impossible to introduce the hand into the uterus and turn a child, even when the promise appears most favourable, without compromising the woman's security to some extent; and if this operation be undertaken after the membranes have been broken some time, and while the uterine parietes are strongly embracing the fœtal body, the peril to her will be greatly aggravated. This will be regulated principally by the degree of contraction that the uterus has taken on itself; and that will in a great measure depend on the length of time that has elapsed since the liquor amnii was evacuated. The danger to the mother consists in the risk that her soft structures—particularly the uterus—should be bruised or lacerated by the operator in the introduction of his hand, or by the irregularities of the fœtal body and limbs, as it revolves round its own axis, in the efforts we make at extraction."

After stating additional objections, the author says:

"There is only one kind of case in which I would be inclined to sanction turning, in consequence of diminished capacity of the pelvic brim:—if the clear available space in the conjugate diameter were about three inches and a quarter, or from that to three inches and a half; if the woman's previous children had all been born dead; if the membranes were still whole,—or the liquor amnii having been evacuated, the uterus had not contracted closely around the child's body, the head being perfectly free above the pelvic brim, not having as yet descended at all into the cavity; and if the attendant were in the habit of performing obstetrical operations, had acquired a certain dexterity in regard to them, and had perfect confidence in himself. Under such circumstances, this means of delivery might be put in practice, as an experimental measure, in the hope of saving the child's life; but even then, not till the patient was informed of the intended operation, its principle explained to her, its objects detailed, and her concurrence and acquiescence fully obtained."—*London Journ. of Medicine, April,* 1852, *p.* 348.

145.—CASES OF ARM-PRESENTATION.

By DR. J. HALL DAVIS, Physician to the Royal Maternity Charity.

[The first case was in a patient aged 32. Seventh child. Previous labours easy. The membranes had given way at half-past six p.m., discharging a large quantity of liquor amnii; the pains continued severe for half an hour, when they ceased. A midwife in attendance sent for a surgeon at half-past seven, who finding the arm and funis presenting, tried to turn, without success, even after a full dose of opium had been given, though he managed to bring the foot to the pelvic brim. Dr. Davis continues:]

At my visit, at half-past ten, I found an arm and foot at the brim of the pelvis,

with coils of the funis without pulsation; the patient not feverish nor plethoric. Thinking that there might yet be a remote chance of saving the child, the cord having only recently ceased to pulsate, and finding turning impracticable at present, I administered chloroform as far as I deemed it judicious to venture, one ounce of the fluid being disposed of, but without the desired effect of relaxing the rigidly contracted uterus. I may say that the specimen of that medicine, judging from its effects upon myself, seemed to be good. I now perforated the axilla, my friend, at my request, keeping the presenting arm on the stretch; I removed the viscera of the thorax through the opening thus made, and by a puncture with my finger and a crotchet, gradually brought the child through the brim, pelvic cavity, and outlet. The pelvis appeared to me deeper than a standard pelvis, which made the axilla less accessible than otherwise it would have been, and rendered the operation a little more difficult. The placenta was quickly thrown off into the vagina, and thence removed within ten minutes after the birth of the child. The child was of average size; full term. The pulse after delivery was 90, of moderate strength. The uterus was left favourably contracted. The patient's puerperium was passed through without a single bad symptom.

Remarks.—Had my friend been called to this patient before, or immediately after the discharge of the waters, in all probability his attempts to turn would have been successful, and the child's life would have been saved. Of the three remedies for the subduction of inordinate uterine action—bleeding, opium, chloroform—the first was contra-indicated by the patient's freedom from pyrexia and plethora, her condition rather below than above par; the second and third sedatives were tried, but without responding in any degree to our wishes. The only resource left for the relief and security of the patient, was delivery by embryotomy, after the manner indicated for a transverse presentation. In the instances of labours with large children presenting transversely, it is, as I have already illustrated, necessary to decapitate before we can succeed in extraction, but the average dimensions of the child in this instance rendered unnecessary that part of the operation.

[The second was a case which Dr. Davis saw at nine p.m., Feb. 12th, 1850, at the Parochial Infirmary; the subject deformed by rickets, of short stature and spare habits, aged 28, in labour of her third child, at full term. Previous labours had been protracted, and terminated in still-births.]

The membranes had broken at midday; there had been much hemorrhage, which, on examination, appeared to have been determined by a partial presentation of the placenta. Attempts at version had been made, on two different occasions, without success, even after the patient had been subjected to the influence of a full dose of opium.

My attempts were alike fruitless, until I had placed the patient under the influence of chloroform, of which six drachms were disposed of, in separate portions, upon a handkerchief, loosely held over the nostrils and mouth. The child was then turned with perfect ease. In performing the operation, I employed the tape-noose to fix the foot at the brim, while with the fingers of the other hand I pressed up the presenting part. The patient was very restless and unmanageable before the exhibition of chloroform; calm afterwards, and during a part of the time that was devoted to the operation, unconscious. The child was still-born, as was to be feared after so long delay and pressure on its circulation. The after-birth followed in less than ten minutes, without hemorrhage. No inconvenience followed the use of the chloroform, beyond vomiting three or four times, what had been previously taken. The patient was left with a well-contracted uterus, and a good pulse.

Remarks.—An earlier resort to the exhibition of chloroform would, in all probability, besides shortening the patient's suspense and suffering, have saved the child's life. Abstaining, as I do, from chloroform from choice, in natural labour, I deem a case of this kind, in the absence of any organic disease or peculiarity to contraindicate its exhibition, admirably adapted for its employment. That agent, it was seen, succeeded in diminishing the spasmodic action of the uteras, opposing the efforts to turn, where a corresponding quantity of opium had entirely failed. In one of my former illustrations, I related a case of placenta prævia, in which I had been consulted, attended by much hemorrhage, where the uterine spasm was so great that turning was quite impracticable. Opium was inadmissible on account

of the peculiarity of the patient's constitution, in reference to that drug. Chloroform was administered at my suggestion to the production of its sedative effect: the operation of turning then became easy, and was completed in time to save the child, which was living and vigorous on the following day, and for aught I know to the contrary, is so at the present time. I have not, in the few cases, nineteen in number, in which I have given this agent, observed any increased tendency to hemorrhage from its use. I have not found it in that experience to diminish strictly parturient action. It has appeared to me that its sedative effects on the uterus have been limited to the subduction of irregular or spasmodic action of the muscular fibres of that organ, which, I take it, is the kind of resistance opposed to our manual proceedings in cases of difficult or impracticable turning.—*Lancet, Jan.* 31, 1852, *p.* 117.

146.—ON THE USE OF GALVANISM IN OBSTETRIC PRACTICE.

By John Hyde Houghton, Esq., Surgeon to the Dispensary, Dudley.

[With the exception of Dr. Simpson, those who have published cases of uterine hemorrhage speak in unvarying terms of the power of galvanism, and would lead us to look to it with confidence in some of the most trying difficulties in obstetric practice. Dr. Radford, of Manchester, speaks in high terms of praise as to its value.]

Case I. Mrs. M., aged 28, a small, delicate, anemic-looking person, is now, July 13, 1847, at the full term of her eighth pregnancy. I saw her at 7 a.m.; she has had premonitory pains for a day or two, but they are now regular and tolerably strong every three or four minutes; the os uteri is three-fourths dilated; membranes entire, soft parts lax and well lubricated; head just descending through the brim. Pain continued regular, with little progress, until eight o'clock, when I ruptured the membranes. The pains then gradually diminished in force and frequency, and at a quarter-past eight had quite left her. Three doses of ergot of rye, stimulants, bandage, and frictions failed to produce the slightest pains; the head had descended into the hollow of the pelvis. I waited until half-past eleven o'clock, when she became very anxious about herself, and begged I would, if possible, hasten the delivery. I therefore sent for the galvanic apparatus, and at twelve o'clock commenced its administration by applying a very feeble current, one pole being placed in the vagina, the other on the abdominal wall; she immediately cried out, "Oh! you are running a pin into me." After three or four minutes she had a very slight pain, and after three or four minutes more one stronger; the pains continued increasing in force and frequency for about twenty minutes, when she was delivered of a small, but healthy child.

The pains produced by the galvanism exactly resembled those of natural labour; and, but for the presence of the apparatus, one would have said she was completing her labour in a natural and favourable manner. The poles were *kept constantly applied.* The uterus soon contracted firmly, and expelled the placenta. The mother and child did quite well.

Previously to her present pregnancy, Mrs. M. had several abortions from ulceration of the cervix uteri, which had yielded to the usual treatment. From this time she enjoyed better health than she had done for years. She got stout, lost her anemic look, and remained quite free from uterine symptoms, which had long troubled her.

Case II. On the 2nd of July, 1848, at 4 a.m., she was again in labour at her full time. I was prevented from attending her myself; and she was attended by Mr. W. W. Tinsley, now of the Sheffield General Infirmary, whom I acquainted with the facts of her previous labour. The following is that gentleman's account of the labour, slightly condensed: "She had been in pain all night, and the pains now came on every five minutes, not strong, and sometimes not quite so often. The membranes were ruptured before I saw her; soft parts relaxed, cool, and moist; os uteri high up, soft, and dilatable, but little dilated; head presenting in the first cranial position. In an hour, little progress being made in the dilatation or descent of the head, I gave her a dose of ergot of rye, which only produced vomiting; in another hour she seemed much the same; and with my hand on the *abdomen I could feel* that the uterus did not contract with the pains, which con-

tinued gradually decreasing, although they never left her entirely. The bandage, friction, &c., were now tried without effect. This state continuing, as I knew that galvanism had been used with effect before, it was again applied. An assistant placed one pole over the fundus on the abdominal wall, and I applied the other at the outlet of the vagina, at the same time watching the progress of the head. At first we could not regulate the power, but at length we got a *continuous stream*, which was gradually increased as far as she could bear, for that only seemed sufficient to produce contraction. In less than ten minutes she complained of the pains getting stronger, and indeed the effect was obvious, for the pains soon became expulsive; the head began to advance, and in about a quarter of an hour made its appearance at the outlet. The poles were now removed, and a natural pain was sufficient to expel the head, and soon afterwards the body. The placenta came away in a few minutes, and the uterus contracted firmly. Her convalescence was quite favourable. During the progress I sometimes moved the pole from the vagina to the sacrum, but the effect seemed to be the same. The chief pain was caused by the pole over the fundus. The galvanism did not seem to produce natural alternating pains, but rather one gradually increasing contraction, which hardly left her until the child was born. The time which elapsed from the first application of the poles until the birth of the child did not exceed one quarter of an hour."

Case III. Mrs. I. was taken in labour September 5, 1847. I saw her at half-past nine, p.m., when I found the os uteri dilated to the size of half-a-crown, soft, and dilatable; head presenting naturally; pains regular, but feeble; the pains continued frequent and feeble until half-past one, at which time the child was born. As the head was passing the outlet a copious gush of blood took place, and blood continued to flow *violently* after the birth of the child. I at once removed the placenta without difficulty; the flooding, however, continued; pressure with the hand caused the uterus to contract, by which the bleeding was *restrained;* a slight drain still continued; at times the uterus relaxed under the hand, when the bleeding immediately returned. Shortly an alarming gush took place, followed by pallor, restlessness, vomiting, and fainting; ordinary means had been already tried, and now cold water was *dashed* freely on the abdomen, without producing any permanent effect. The uterus *would not contract effectually*, and the bleeding continued. I now sent for the galvanic apparatus, and during three quarters of an hour which elapsed until its arrival, I kept firm pressure on the uterus with my own hands, by which means the flooding was restrained. The organ, however, showed *a constant disposition to relax*, and consequently *the bleeding to return*. The state of the patient had become very alarming; the poles were applied without delay; and they soon produced *firm and permanent contraction, and complete suppression of the bleeding*. For a short time I kept my own hand firmly on the uterus, that no unobserved relaxation and internal flooding might occur. Shortly afterwards I desired an assistant (who had much midwifery experience) to feel if the contraction continued; the moment he placed his hand on the abdomen he looked quite astonished, and said he had never felt so firm a uterus. Her recovery was gradual, and without accident or drawback, more than that which resulted from loss of blood. I have since attended her in an easy and natural labour, in which there was no accident or complication.

[In a fourth case, pains commenced on the morning of the 8th of May, 1851, at 2 o'clock; at 9 o'clock the os uteri was fully dilated; at noon it remained in the same state, the membranes were ruptured, and two doses of ergot given, which slightly increased the pains, but the effect soon passed off. At half-past six in the evening she remained in the same state,—the head low down, the pains slight and infrequent. Galvanism was now resorted to, but the result was by no means so decided as in the former cases, though there can be no doubt that it much hastened the completion of the labour.]

I shall add a resumé of all the cases I can find in which galvanism has been used in midwifery practice.

Mr. Dorrington.—1. Internal hemorrhage during labour; the pains had subsided; *os very rigid*, size of half-a-crown; uterus quite lax; there were all the symptoms of exhaustion from flooding present. Laudanum was given; the bandage applied; and then half a drachm of secale cornutum administered. This pro-

duced pains; the head bore down on the os, but between the pains the uterus was very lax. Galvanism was applied. In a short time "*tonic contractions of the uterus had been called into play.*" It was resumed, and "*strong contractions took place at once.*" After using it for twenty minutes it was finally left off. "So firm a state of tonic contraction having been induced, that we considered it safe to leave the woman," ordinary nourishment, &c. &c. The flooding ceased; pains soon commenced, and labour was completed in four hours afterwards. The following day she was going on well, but died of an "obscure peritoneal affection" some days after.

2. A. B.; ninth pregnancy; full time. A copious gush of blood took place at 12, p. m. Mr. Dorrington saw her at 2, a. m.; she was faint; pulse feeble; no hemorrhage or labour pains; the uterus was lax. "*The firmest tonic contractions were induced the moment galvanism was applied.*" "*No more flooding occurred.*"

3. Placenta prævia; flooding without obvious cause between seventh and eighth month. She was seen at the eighth month; slight flooding going on; os the size of a penny piece; membranes entire; slight pains. The membranes were ruptured, and the galvanism applied; "*good strong uterine action set in at once.*" The hemorrhage was very slight, and did not return; the child was born in three hours after. The use of galvanism had to be resumed, as the pains subsided a little.

4. A case of twins, at seventh month; first child born; no pains for an hour after; they then commenced very slightly; in four hours they were very slight, and recurred only every half hour. Galvanism was used, "*the effect was immediate, strong labour pains coming on, and continuing while the current was complete.*" The child was born in a quarter of an hour.

5. Induction of premature labour at eighth month. "The uterus hardened under the application, and she felt labour pain, but this lasted only while the shock was continued."

Mr. CLARKE.—Two cases of uterine inaction, in which galvanism was used with success and safety to both mother and child. (Mentioned in the Dublin Hospital Gazette, March 1, 1845.)

Mr. CLEVELAND.—Atony in previous labours; pains commenced on Sunday, and continued until Wednesday evening, when they abated, but did not cease. Ergot, &c., failed to produce pain; symptoms of exhaustion then set in. Electro-galvanism was applied. "*A very decided effect was soon produced.*" "*Regular, strong, and frequent pains came on; and in a quarter of an hour a living child and the placenta were expelled with the least degree of hemorrhage I ever witnessed. Immediate and firm contraction of the uterus followed.*"

The following cases, by Mr. Demsey, I have extracted from Dr. Golding Bird's lectures:—

1. Hemorrhage after the birth of child in a natural labour. Ergot, cold, friction, introducing the hand, and extracting placenta, failed to produce action. After five minutes' application of galvanism, "*energetic contractions ensued, emptying the uterus. In a minute or two the uterus was felt firmly contracted,*" and all "*danger at an end.*" "The patient quickly recovered."

2. Profuse flooding with each pain for six hours; patient much exhausted; cessation of pains for three-quarters of an hour. No fœtal pulsation could be heard; os size of crown-piece, soft, dilatable; placenta presenting, and beyond this the head; application of poles seven minutes, *when pains commenced; application suspended ten minutes; no indication of pain. Repeated applications and suspensions—first for ten minutes and then for five minutes—for forty-five minutes, when the child was born.* Renewal of application was necessary for the expulsion of the placenta.

3. Almost identical with last.

4, 5, 6, 7. Galvanism, for post-placental hemorrhage; "*the loss ceased almost immediately on passing a current through the uterus.*"

8. Lingering labour, from atony; labour protracted nearly thirty hours; pelvis capacious, well formed; pains extremely feeble, and at long intervals; fainting at short intervals; no pains for nearly three hours; os dilated; fœtal heart heard. On first application, *slight pains;* repeated after an interval of five minutes, *pains decided and energetic;* galvanism applied every five minutes; child born in forty-*five minutes.* Ergot, &c., had been previously used in vain.

9. Hemorrhage in miscarriage, without uterine action. Pregnancy at the third month; flooding (from fright) of three days' duration; os rigid and unyielding; no pains at all; a portion of placenta felt protruding; constant drain going on; acetate of lead, ice, and cold lotions, ineffectual after seven hours' trial; four doses of gallic acid then produced no effect; ergot every twenty minutes without benefit. Galvanism was now used; no effect for twenty-eight minutes, the contractions *then became quick and forcible, and the ovum was expelled in sixty-eight minutes.* Convalescence was speedy.

10. Induction of premature labour at the seventh month; puncture of membranes; no pains in forty-eight hours. Galvanism was then used *thus:* five minutes' application, and ten minutes' interval; after the third application, slight, transient, grinding pains came on. Suspension for half-an-hour; cessation of the pains; application resumed every ten minutes for forty minutes; pains became strong and regular; it was now discontinued, but the pains gradually increased, and expelled the head.

Dr. Radford.—1. Hemorrhage at eighth month. Uterus flaccid and inactive. Rupturing the membranes, and other ordinary means, failed to produce action. "*From the moment the circle was completed, uterine pain was excited, and a bearing down effort was produced.*" Tonic contraction took place; the *flooding was arrested, and did not recur,* and the labour was completed favourably.

2. A fourth labour: after full dilatation and rupture of the membranes, the pains ceased; constant discharge of blood for six hours; the uterus could be felt flaccid through the abdominal parietes. Galvanism was now tried; a slight power was at first used, and gradually increased; the poles were applied on various parts of the abdomen. The beneficial influence of the agent was soon apparent; the *atonic state of the uterus was soon changed; the parietes became firmer; pains, at first grinding* and slight, *became powerfully expulsive,* and the child was born one hour after the commencement of the operation. *The uterus contracted firmly; the discharge of blood ceased as soon as the uterus began to contract, and there was no further flooding.*

Dr. Radford observes: "The powerful and sanitary influence of galvanism was most decidedly obtained in this case, and the great advantage of this agent is, that its effects may be carried to any degree, from at first only exciting the uterus so to contract that its diameter may be diminished, and that its tissues come to be applied to the surface of the child. This, however, may be so increased as to effect the expulsion of child and placenta."

Dr. Simpson's eight cases are recorded with unusual accuracy, and, apparently, every care was taken to avoid any erroneous or fallacious result being produced; and, in order to insure this, he noted first the duration of the pain, and then the duration of the interval,—

1st. Before the application of the wires.

2nd. After the application of the wires, but before the circle was complete.

3rd. Whilst the wires were applied, and the circle complete.

4th. After the removal of the wires.

He thus, it seems, took the best means to ascertain accurately the extent to which uterine action was excited, and to avoid a fallacy which might be caused by the influence of emotion, &c.

I shall not enumerate Dr. Simpson's cases at length, as I have done those previously recorded, because I have a *resumé* of them in his own words, in which, of course, the results are truly stated; and as my business is with the results simply, there is not any necessity for my giving them in detail. The others were so given that I might use as much as possible the words of the authors, and thus insure accuracy.

"In one instance the pains were more frequent in their recurrence, but shorter in their duration, during the application of galvanism. In five other cases, the employment of galvanism neither increased the average frequency of the pains nor their average duration. In one case the pains ceased while the galvanism was applied; and returned upon its removal; in the instance which I have last detailed the uterine action ceased while the galvanism was applied, and did not return upon the withdrawal of the galvanic action, nor for twenty-four hours subsequently. There was no reason whatever at the time to expect this as a probable occurrence,

independently of the galvanism. But even admitting, for the sake of argument, that the cessation of the uterine action was not the result of the galvanic influence used, still the fact ● amply sufficient to show that the galvanic current had not, at least, the power either of increasing the pains, or of continuing or maintaining them when they offered to fail. It may be proper to add, that during the galvanic action I did not find (in any of the experiments) between the *clonic* uterine contractions or pains, any evidence whatever of unusal *tonic* contractions of the uterus, as shown either by any degree of hardness on the general uterine tumour, or by any degree of tension in the pressure of the bag of membranes, or the child's head, against the cervix uteri."

Thus the results of the foregoing thirty-two cases, in which galvanism has been used in obstetric practice, have been cited, and it appears that its effects were—

Decided in 24 cases.
Equivocal in 1 „
Negative in 7 „

So that in 75 per cent. the effects of the agent were clearly manifest:—In eight cases, for hemorrhage before expulsion of the ovum; in six cases for hemorrhage after expulsion of the ovum; in eight cases, for atony of the uterus; and in two, for induction of premature labour.

The cases recorded have occurred in the practice of eight observers. Of these, seven bear unvarying testimony to its powers, whilst one altogether doubts it. Indeed, all the cases in which the results were equivocal or nugatory were observed by him; and he has not observed a single case in which the result was satisfactory.

Hence, the facts seem to be in such strange contrast that one is almost forced to the conclusion that in Dr. Simpson's cases some undetected source of fallacy must have existed; and Dr. Golding Bird seems to entertain a somewhat similar opinion, for in his 'Lectures on Galvanism' he says: "I cannot for one moment admit the validity of his (Dr. Simpson's) opinion when opposed to the facts of Dr. Radford, Dr. Lever, and others; but would endeavour to show the mode in which these opposite statements appear to admit of reconciliation. This is founded on the opposite effects of currents, according as they follow the course of the centripetal or centrifugal nerves. Now in the magneto-electric coil, in which currents are excited by repeatedly breaking contact by a vibrating bar, we have two currents moving in opposite directions, to each of which the patient who is subject to the experiment becomes submitted. Now, these currents are of an unequal strength, and if the most energetic, that on breaking contact, be passed in the direction of the *vis nervosa*, it will produce painful contractions, which, the moment it passes in the opposite direction, will become relaxed; for a direct current tends to produce contraction, an inverse current, paralysis. Hence, I should urge the accoucheur not to employ the apparatus in which both these currents are produced, but simply the *single current* machine. In using this I would suggest the positive conductor to be placed over the lumbosacral region, and the other to be carried over the abdominal surface, with a gentle friction. In this way powerful uterine contractions may be easily excited."

I am not sufficiently master of the subject to offer any opinion as to the theoretical truth of this explanation; but, as referring to the case in question, it must fail as an explanation of the want of success in Dr. Simpson's cases, for Dr. Simpson says that "he used an instrument similar to the one used by Dr. Radford, and made by the same makers." Hence it ought to have produced the same sort of currents, whatever they were, and the same results.—*Dublin Quarterly Journal of Med. Science, Feb.* 1852, *p.* 11.

[There can be no doubt that galvanism may be considered as a most important addition to our list of remedies in cases of flooding, either before or after delivery, or in cases of atony of the uterus, though the results are more important in cases of flooding than in simple atony. Speaking of the ergot of rye, Mr. Houghton remarks, that in the last 330 cases of labour he has attended, he has given the ergot in 38: its effects were decided in 26; equivocal in 6; and nil in 7. Although this is a mere statement from a few cases, yet it shows that the ergot failed or was *equivocal in just* one-third of the cases; while galvanism was quite successful in *three-fourths of* the cases in which it was employed. Should Pulvermacher's

hydro-electric chain batteries prove effectual, it will be a most convenient method of applying galvanism in obstetric practice.]

147.—CASE OF INVERSION OF THE UTERUS.

By J. G. Forbes, Esq.

[The following case was read before the Royal Medical and Chirurgical Society.]

The patient, a lady, twenty-five years of age, came under the care of Dr. Robert Lee and the author, in December, 1850, being in appearance intensely anæmic, and suffering from the usual symptoms attendant upon this state, as well as from periodical attacks of profuse uterine hemorrhage. On examination per vaginam, a tumour of a pyramidal shape was detected projecting for about two inches through the os uteri, with which it appeared in close connexion throughout its entire circumference. Her confinement had taken place in the country in April of the same year, and an account of it was furnished to the author by her medical attendants. Owing to the cessation of pain, ergot was administered, the os being fully dilated; and a loop of the cord having descended in advance of the head, the forceps were employed to complete the delivery. The infant was still-born. Nothing unusual was apprehended until several days after the labour, when the patient stated that during the action of the bowels on the third day "something had come down." It was now discovered that the uterus was inverted, having descended to within an inch or two of the labia. Re-inversion was found to be impracticable. Slight sanguineous discharge continued up to the 12th July, on which day profuse hemorrhage took place. This recurred afterwards at each catamenial period, the discharge being often of a serious character, and was accompanied by hysteria, headache, vomiting, constipation, and great exhaustion. On the 18th December, Dr. Locock was called in consultation, and the propriety of applying a ligature around the tumour for the purpose of extirpating it was discussed, but owing to the diminution of hemorrhage at that time it was not done. The pain produced by pressure on the tumour, precluded all hope of accomplishing its reduction. The treatment adopted was therefore only palliative. In April, 1851, the patient returned to the country, when, after the lapse of some months, her health began to improve. The hemorrhage also ceased, for the last three catamenial periods of her life were passed over with only a moderate amount of discharge. The period which commenced early in November was attended with intense headache, which, in spite of remedies, increased; erysipelatous redness of one side of the head and neck came on, and the patient gradually passed into a state of coma, under which she sank and died on the 10th instant. A post-mortem examination disclosed incomplete inversion of the uterus, the tumour not being constricted by the os. The left ovary was enlarged, and contained a cyst filled with sanguineous fluid. The paper then contained some remarks on the question of attempting the extirpation of the inverted uterus in such cases by operation, a few observations being made, first on those cases in which no operative proceeding was resorted to, and then upon the several modes of operating which have been practised, and their results. Some cases, it was stated, underwent spontaneous re-inversion, and in others the vitality of the tumour was destroyed by inflammation or by the constriction of the os uteri, and the patients recovered. The difficulty in accomplishing the reduction of the uterus after the lapse of a few hours, was then spoken of, and a few cases were quoted in which it was effected at later periods. The duration of life under inversion of the uterus was then considered, and Mr. Crosse's statistics were referred to. Cases in which extirpation of the uterus was performed by excision, a ligature alone, and by both combined, were next quoted, and a summary was given of the table appended to the paper. In the latter, which contained brief details of thirty-three cases, collected from different sources, it appeared that nineteen were successfully treated by ligature, and five unsuccessfully (three died); one successfully and two unsuccessfully by excision; and five successfully and one unsuccessfully by ligature and excision combined.

[Mr. Arnott said as it was desirable the experience of members should be stated upon this subject, he would mention the following case:]

Three years since, he was consulted on a case, which was supposed to be a polypus of the uterus by a distinguished accoucheur who suggested its removal. Two other gentlemen had also been consulted: one pronounced the case to be polypus, the other suspected it to be inversion of the womb. With this latter opinion he (Mr. Arnott) agreed. The history of the case was simple:—The woman was twenty-seven years of age, and had been confined twelve months previously. There was some tugging at the navel string, but after three tugs it came away. There was not excessive hemorrhage at the time, but more or less bleeding continued for four months, at the end of which time an examination was made, none having been instituted before; and then a difference of opinion existed as to the presence of a growth, or of inversion of the uterus. A ligature, however, was applied, and the bleeding ceased. The woman did not get well, suffered much at the menstrual period, and on laughing or making any exertion, something protruded from the vagina. She now came to London, and he (Mr. Arnott) saw her under the circumstances above mentioned. On the first examination, the tumour had every appearance of a polypus about the size of a goose's egg, firm, with a pedicle. There was an offensive discharge, not menstrual, which had continued for seven weeks, but there was little difference in the sensibility of the part. He doubted if it was a polypus, but thought it might be so. He felt the pedicle encircled by the os uteri. The surface was livid at some portions, and tender at one part. This was not sufficient to determine that it was not a polypus. Another opinion was now sought in the case, and the accoucheur called in pronounced it to be a polypus. It was now determined that the patient should be placed under chloroform, the tumour brought down, and if a polypus, that it should be removed. When brought down, the tumour exactly resembled a polypus, and on passing the finger into the bowel, what was supposed to be the uterus could be distinctly felt in the rectum. Mr. Arnott, however, was determined to act with caution, and therefore made an incision into the pedicle, when it was at once detected to be an inverted uterus. A ligature was applied, but as hemorrhage continued below its seat, another, and even a third, was used before the bleeding ceased. The parts were then returned. The woman died of peritonitis. This case showed the necessity of caution in our diagnosis of these cases, and the difficulty there was of restraining hemorrhage. Now, what was the tumour felt through the rectum when the tumour was down? This was, no doubt, the bulging out of the neck of the uterus, as if it were the whole body of that organ. This case constituted his sole experience of these cases. Before the occurrence of the one he had just related, he had considered that the best mode of proceeding was to tighten the ligature at once, but now he thought the employment of a metallic ligature preferable, which should be slowly tightened. Experience, however, was wanted on this point. It was not easy to procure adhesion in these cases, and hence it had been thought better to tighten the ligature at once. He had seen another case in which there was copious bleeding, and an operation was suggested; the patient would not consent, and died worn out.

Dr. MURPHY made some remarks on the difficulty of diagnosis in these cases, and mentioned an instance related by Velpeau, in which a polypus was mistaken for inverted uterus. The accident was usually the result of want of attention in the first instance, and never occurred in the hospitals of Dublin and the continent, where great care was employed. In the cases recorded, there was some obscurity as to the progress of the last labour and the cause of the displacement. He related a case recorded by Mr. Crosse, and occurring in the practice of Mr. Martineau, at Norwich, to show that, by early and judicious treatment, the displacement might be remedied. In this instance a lady, whose labours were remarkably rapid, was seized with a violent pain whilst standing up, by which the child was expelled. She had severe pains afterwards, and on a careful examination being instituted, the uterus was found to be partially inverted, and was replaced. If the uterus became quite inverted it was most difficult to replace it, from the great contraction which existed. Much mischief might ensue from persevering attempts to reduce the displacement. It was mentioned, in some cases, that the uterus had gradually restored itself to its right position, but he, Dr. Murphy, believed that, in these *cases, it was simply* a reduction in the size of the tumour, and not a replacement. *If the first few hours* passed after the accident, and the uterus was not restored, it

was impossible to replace it by mechanical means. Then the question presented itself, is it desirable to remove the uterus, or to leave the case to nature? On this point it was a mere balance of the chances of life. He could not help thinking, however, that with further improvements in surgery, the organ would be removed as easily as any other. Dr. Johnson, of Dublin, had operated twice or thrice successfully. He employed silver ligatures.

Dr. TYLER SMITH was sorry to hear Dr. Murphy attribute the occurrence of inversion in all cases to some fault on the part of the medical attendant. He was convinced it might occur without any negligence or interference on the part of the accoucheur to account for it. The uterus had been known to invert itself suddenly after labour, when the woman was lying perfectly still; it had also happened in women who had not been recently delivered; and there were cases on record in which even the virgin uterus had inverted itself spontaneously. An opinion very generally obtained that the uterus is always inverted mechanically by injudicious tugging at the funis, but in this he could not concur. On the contrary, he considered that inversion was usually a muscular act of the uterus itself, similar to the action of the intestine which caused intussusception. He would suggest the use of chloroform in recent cases of inversion, from its power of diminishing uterine contraction, and from having had cases of prolonged retention of the placenta, with spasmodic contraction of the os and cervix uteri, yielding to no other means, which had been overcome by the use of chloroform. It was melancholy to reflect that a mal-position so slight as that which existed in the preparation upon the table of the Society, should cause almost inevitable death. While listening to the details, it occurred to him whether chloroform would have been serviceable had it been resorted to when the reduction of the inversion was attempted, with a view to allaying the pain and relaxing the tissues, as had been done in strangulated hernia while applying the taxis. In veterinary practice, inversion of the cornua of the uterus was not an uncommon accident, and recently an instrument had been used, consisting of a ball affixed to a rod, for the purpose of reducing the displacement, and, as it was said, with great success. Some modification of this might probably be of use in obstetric practice, as such an instrument would give the practitioner more command over the uterus than could be exerted by the hand alone.

[Dr. LOCOCK said, that cases were so rare that no single individual, from experience, could come to general conclusions. In one instance he had seen, the uterus had been inverted nearly three days before the accident was discovered. Efforts to replace failed; great pain and vomiting being brought on. Three or four days after, the attempt was renewed, but entirely failed, the parts having become so much contracted.]

If chloroform were to be resorted to in such a case, it must be employed very soon after the accident, because, when the inversion had existed any length of time, the inverted organ was no longer elastic or spasmodically contracted, but became perfectly solid, and it was nearly impossible to replace it without great danger of rupturing the vagina. To be successful, the effort should be made soon after delivery. In one case he had succeeded in replacing it an hour or an hour and a half after the accident. In proof that the uterus may be inverted spontaneously, he related a case in which the woman had no one near her, and there had been no pulling at the cord. The child lay in the bed, and the umbilical cord was not separated. The uterus, however, was completely inverted. He detached the placenta from the then convex surface of the uterus, which he had no difficulty in replacing. The patient did well. He recollected a lamentable case which had occurred to him about sixteen years ago. He was called, and found two medical men attempting to bring away what they supposed to be the placenta, but which was in reality an inverted uterus. The patient died before he arrived, and all he could do, to save appearances, was to remove the placenta and replace the womb. He had seen five cases of inversion of the uterus; of these, four were preceded by attempts to remove the placenta by pulling at the cord, and the fifth case was the one he referred to as occurring spontaneously, and which he succeeded in replacing. The uterus would also now and then become inverted in consequence of the presence of a polypus. Such a case was related by the late Dr. Hooper. The preparation was, or ought to be, in the museum of St. Thomas's Hospital.

In this case the uterus was partially inverted, from the expulsive efforts caused by a polypus little bigger than a small cherry. It could not have been the weight of the polypus, but the irritation produced by it, which caused the inversion. The woman died of uterine hemorrhage. These were all the particulars of the case with which he was acquainted; the case had occurred in the St. Marylebone Infirmary. He (Dr. Locock) had seen small polypi produce efforts of expulsion similar to those of labour. In one case he had found, on passing the finger, the uterus to be partially inverted from this cause. On removing the polypus, the uterus became replaced suddenly, as if with a spring. He thought, with respect to the removal of the uterus in case of chronic inversion, there was much room to doubt its propriety: patients died of anæmia in these cases sometimes, as in Mr. Forbes's cases, and not from the inversion of the uterus alone. The case related by Mr. Arnott was unfavourable for operative procedure. There had been much manipulation before the application of the ligature; there was pain, and the surface of the inverted uterus was coated over by a thick layer of half-purulent lymph of an unhealthy character. The patient herself was not healthy. This, therefore, was not a fair case to arrive at a decision upon. When, however, we found so many patients lived in comparative comfort after the cessation of menstruation, that so much could be done with styptics and other local measures, as well as by constitutional treatment, an operation should not be lightly resorted to.—*Lancet, April* 3, 1852, *p.* 331.

148.—NEW POLYPUS CANULA.

By Dr. Oldham.

[In relating a case of large polypus of the uterus, Dr. Oldham describes the instrument he used as follows:]

The instrument which was used in this case, was a modification of Gooch's double canula, which, in my experience, is by far the best when the polypus is large, high up, and closely girthed by the vagina. The mode of applying the ligature was by carrying the two canulæ together in front of the polypus, and beyond its body, then separating each canula, and directing them simultaneously backwards, so as to let them meet at the back part of the polypus, by which time the growth was noosed. Dr. Gooch recommends one canula to be separated, and to be carried round the polypus until it reaches the starting point where the other has been fixed. This is a far more difficult process, and I have seen an instance very closely resembling the above detailed case, where the physician abandoned the operation, after trying it for a length of time, from keeping too closely to Dr. Gooch's rules. The polypus in the case related was easily secured, and the ligature was drawn tight without producing pain. When the polypus is somewhat smaller and lower down, the ligature may speedily be passed around it, by placing the index and second fingers, each upon one of the canulæ, then separating the fingers and sweeping them around the polypus, so as to meet at the opposite side, the canulæ being pressed before them. Whenever it is practicable, I employ a single canula, through which the whipcord ligature is run, because it is a simpler instrument, and it clips the pedicle better and promotes its earlier detachment. But it is only in smaller polypi than that which is now being noticed that the noose can be adjusted by the fingers. One of the greatest practical improvements in the instrument for noosing polypi is in the contrivance for tightening the ligature, so as to exert considerable power over it, and to keep it well strained upon the pedicle. Mr. Durrock some time since completed an instrument for me, under my directions, which fulfils these indications extremely well. The cross stem on which the ligature is wound is moved by a screw at the end of the instrument,

acting through two cogged wheels, placed one at the side of the box and the other at the lowest part. A bolt at the side fixes the wheels, so that the ligature cannot slip back. By this instrument great power may be easily used, and it is altogether convenient and useful.—*Med. Times and Gazette, March* 6, 1852, *p.* 244.

149.—ON THE DIAGNOSIS, TREATMENT, AND PATHOLOGY OF OVARIAN TUMOURS.

By Dr. Frederick Bird, Lecturer on Midwifery and the Diseases of Women at Westminster Hospital.

[It is to be observed that where the growth of ovarian tumours is slow, the health may not be for a considerable time influenced; but where the growth is rapid in its progress, urgent symptoms present themselves, and there is concomitant destruction of general health. The previous history of an ovarian tumour affords us little help in its diagnosis except the absolute duration of the disease, and whether in its commencement inflammation or congestion of the ovarium had occurred.]

The unilateral position of the diseased ovarium has been adverted to as indicative rather of a complication of the disease than as diagnostic of its character. In the great proportion of instances, the tumour will be found from the earliest period of its detection to have been centrally placed, and, in the more advanced period of the disease, when abdominal distension has become considerable, I do not think I have ever observed unilateral position of the tumour, even in a slight degree, unless previous peritoneal inflammation had effected adhesion, by which the morbid growth was prevented from falling into that position in the abdomen which it would otherwise have occupied; recording an exception, however, in those cases in which a more or less solid tumour is at its inferior portion impacted in such a manner in the pelvis as to prevent the upper or abdominal part from occupying a central direction. Of the one hundred cases taken to illustrate these remarks, and to which reference has been already made, it was found that the tumour had preserved a central position in sixty-eight examples; in fourteen no sufficiently accurate statement could be obtained, and in the residual eighteen the tumour was unilateral. Of these eighteen cases, it was subsequently proved, in the course of treatment or autopsy, that peritoneal adhesions were present.

Inquiries into the hereditary tendencies to disease will discover in a large number of cases the circumstances of certain members of their family having died from phthisis, and the strumous diathesis will probably be remarked in the sufferers themselves; but there are also many instances in which no such fact can be noted. So is it with reference to age and social position; uncertainty applies to both, and in the search for any peculiar features by which an ovarian tumour may be recognised, nothing is discovered in the general symptoms or condition by which its existence can be rendered more than probable. All that at present can be derived from the analysis of cases seems to be the following:—

(*a*) Ovarian tumour is most frequently found in those of strumous diathesis, and phthisis very commonly exists in the collateral branches of the family, and sometimes in the patient herself.

(*b*) Ovarian tumour is more frequent between the ages of 25 and 35 than at any other period of life.

(*c*) Ovarian tumour is more common in those who have been married than in the unmarried.

(*d*) Ovarian tumour is commonly associated with sterility.

(*e*) Ovarian tumour does not necessarily give rise to any symptoms beyond abdominal distension, until very large size has been attained, unless the development and rate of increase has been very rapid, or peritoneal adhesion or pelvic impaction has caused mechanical pressure upon adjacent structures.

Useful as the preceding facts may sometimes be, it is yet obvious that they are wholly insufficient in themselves to conduct to a correct opinion, and the diagnosis has still to be supported by more certain data.

Ovarian tumour, whether formed of one principal cyst, constituting what is not very correctly called the unilocular species, or of many cysts, as in the multilocular type, may vary very greatly in size and somewhat in shape, but is usually referable

to the same figure,—the ovoid, having the fundus more spheroidal, the base more pyriform, or at least forming an arc of a lesser circle than that described by the former,—the outline is generally regular, unless solid matter, or masses of condensed secondary cysts growing internally from the walls of the primal sac, project outwards and produce a nodulose form; but, however large the hard portions of a tumour may be, if fluid be also contained within, they seldom cause irregular prominence, and often cannot be felt until after the cystic secretion has been removed by the trocar, when the original figure of the tumour becomes lost. Shape, then, becomes one element in the diagnostic examination, and is often of much value.

A certain relative position is also possessed by tumours of the ovary, which they very rarely lose. Unless complicated with peritoneal adhesions, by which the tumour may be fixed unilaterally, that position is central with regard to the lateral regions of the abdomen. The tumour generally rests more or less upon the pelvic brim, and is placed anteriorly to every viscus in the abdomen, with the exception of the bladder, which is lodged in a somewhat triangular space above the symphysis pubis formed by the divergence of the lesser curve of the tumour as it rests upon and is supported by the greater curve formed by the outstretched recti abdominis muscles. The space thus left is often very small, and but little room is permitted for the expansion of the bladder under urinary accumulation, and hence the common symptom of frequent micturition. The relative positions of the ovarium in health and in disease thus become changed; in the former state lying inferiorly and posteriorly to the uterine fundus, and in the latter enlarged by the tumour rising above it, and soon passing forward, leaves the uterus to occupy an inferior and posterior situation. But although, from greatly increased size, the ovarium ultimately becomes anterior to the uterus, yet is the position of the latter organ often otherwise altered—a circumstance dependent upon the variable space interposed between itself and the base of the morbid growth, constituting what is termed a long or a short pedicle; and it will be hereafter shown, that the altered position and direction of the uterus may afford important aid in determining the comparative safety or danger of extirpation. Occupying a relatively anterior position, no alteration in the condition of the other abdominal organs seems to affect that of the enlarged ovarium, and in those examples in which pregnancy supervenes upon ovarian tumour, the uterus still remains behind it, even until the full completion of gestation. On this almost unvarying position of the tumour depends the diagnostic importance of percussion, which ranks as one of the most valuable means brought to the investigation of ovarian disease.

Unless early peritonitis has occurred, which, by its consequent adhesions, has bound down and united the small intestines either to the tumour or to each other, they are gradually carried upwards by the increasing size of the diseased growth, and supported by its fundus, and at last compressed into the hypochondriac regions, there to remain (often performing their functions in a very diminished space) until the removal of the fluid portion of the tumour, or its complete excision, allows them to regain their former and natural position. Frequently their long previous compression prevents them from quickly doing so, and several days often elapse before they escape from the small hypochondrial spaces in which they had become placed, and sometimes not until purgative action has stimulated them to augmented movement; then they descend, and for a time give rise to more inconvenience than when previously displaced. The transverse portion of the colon also, which, when the tumour is of moderately small size, may often be detected resting upon its fundus, is at last, under greater cystic distension, made to take a position posteriorly, and falls to a plane lower than that of the fundus of the tumour, drawing with it the omentum, which, unless of unusual length, or adherent, is seldom found interposed between the anterior wall of the tumour and opposed abdominal surface. Under flatulent distension, the stomach, and probably transverse colon also, rest upon, and are supported by, the upper margin of the tumour, and thus the greater part of the abdomen is distended by a morbid growth of a certain figure, which, placed anteriorly, rests, to the exclusion of other viscera, against the anterior wall of the abdomen, and becomes bounded above, and in its hypochondrial regions, by hollow and commonly flatulent organs, while in its lateral boundaries are placed *ascending* and descending portions of the great and hollow intestine. Perhaps no *condition could* be more favourable for the employment of percussion as a diag-

nostic means, and, accordingly, it is found to possess much utility. By its aid the boundaries of the tumour can be distinctly ascertained. Commencing by accurately percussing below, and traversing inch by inch the whole mesian line, uniform and marked dulness is elicited, until a spot is reached at or near to the ensiform cartilage, varying according to the size of the tumour, at which the dulness is abruptly changed for the tympanitic resonance, and so clear and distinct is this line of demarcation in the great majority of examples, that the upper boundary of the tumour can be readily mapped out. If next, the same careful percussion be made parallel with the mesian line, from below upwards, on either side of the abdomen, through the lateral regions, the same dulness will be remarked, until a spot near to or below the cartilages of the ribs is reached; then dulness ceases as abruptly as before. This spot on either side of the abdomen will be found to be an inch or two lower than that at which in the mesian line dulness on percussion ceased: a line traversing these three spots will describe a curve—it is the fundus of the tumour. If the percussion be now made in a line on either side from the linea alba outwards, into the lumbar regions, and a longitudinal boundary between dulness and resonance be thus ascertained by percussion, less marked it may be than in the former examination, but still evident, such boundary will mark the position of the lateral portions of that space which is found dull on percussion, they will unite with the curve already detected above, and thus form an ovoid outline—the outline of the ovarian tumour. No position of the patient will alter the position of these lines of dulness and resonance,—in standing, or in the prone or supine positions, they remain unchanged.—*Med. Times and Gazette, March* 6, 1852, *p.* 235.

150.—*On the Merits and Demerits of the Ovarian Section.*—[The following are the conclusions which the 'British and Foreign Medico-Chirurgical' Reviewer arrives at after a careful consideration of the whole of the cases bearing upon this point, although they must only be considered on the whole as approximations to truth.]

1st. That in any case in which it is considered advisable to remove an ovarian tumour, it is justifiable to make a small preliminary incision into the abdomen, for the purpose of determining whether the tumour be adherent or not.

2nd. If the tumour be adherent, the incision is to be immediately closed entirely, or to such an extent as merely to leave an aperture the size of that made by an ordinary trocar, and we may then expect that this operation will not, on the average, be followed by much more fatal results than common tapping.

3rd. That where the tumour consists of a simple cyst or cysts, with but small solid deposit, it may be extirpated with as good a chance of success as attends the performance of the more serious surgical operations, and with the further prospect of the cure remaining permanent.

4th. The existence of much solid deposit, or of extensive adhesions, absolutely forbids the operation, which should be brought to a termination immediately upon the discovery of either.

Finally, we may add our belief, that the plan proposed by Mr. Wilson, of tying each bleeding vessel separately, so as to dispense with the ligature round the pedicle, is an important improvement; and, that, if experience should show that it is sufficient for the cure of the disease to remove only a part of the cyst, the operation will be rendered considerably more hopeful.—*Brit. and For. Medico-Chir. Review, Jan.* 1852, *p.* 237.

151.—*On Ovarian Dropsy—Tapping—Partial Removal of the Cyst.* Under the care of Mr. I. B. Brown, at St. Mary's Hospital.—[Before Mr. Brown operated in this case, he stated that the cyst was very thin, and that he would excise a portion of it, after tapping, and leave the fluids, furthermore secreted, to be expelled by other channels.]

The patient is a woman about forty years of age, following the occupation of servant, and who has been suffering from ovarian dropsy for several years past. She had spent some time in another hospital, whence she was discharged as incu-

rable, being told that tapping would be of no benefit to her: she, therefore, now objected to this ordinary means of removing the fluid.

Chloroform was administered by Dr. Snow, first with his inhaler, and afterwards with the simple sponge, the insensibility remaining complete all through the operation. The patient's abdomen is very prominent and pendulous, the skin being of a rather dark tinge, and permeated with large veins. Mr. Brown made his incision in the mesial line, commencing a little below the umbilicus and descending in a straight direction for about four inches. All the layers, down to the peritoneum, were carefully divided, and the latter slit open with the assistance of the director. The cyst, of a light grey colour, now came into view, bulged out by the fluid; and Mr. Brown passed his finger easily between the cyst and parietes of the abdomen. It was now plain that no adhesions existed between these parts, for the whole hand of the operator was easily glided all round the cyst between the latter and the walls of the abdomen. It struck some of the spectators that this great freedom from adhesions, might, perhaps, be favourable to the complete removal of the cyst. Mr. Brown now introduced the trocar and canula in the centre of the exposed portion of the cyst, and evacuated about a gallon of very clear limpid fluid, the cyst being all this while held by an assistant with a vulsellum, so as to prevent its collapse. After the removal of the fluid, Mr. Brown raised, with the vulsellum, a portion of the cyst about the size of the palm of the hand, and removed it by a horizontal section, with the scalpel.

A little fluid which had collected during this process, was now allowed to escape, and the cyst pushed back into the abdomen. The margins of the cutaneous section were brought together and secured by six sutures, a compress applied, and a wide roller placed around the abdomen. Very little blood was lost during the operation, and none at all on the section of the cyst.

When the patient was removed, Mr. Brown stated that his reason for adopting this line of practice was, that cysts which spontaneously burst, have been known to give patients no further uneasiness, the fluid being probably, after the collapse of the cyst, taken up by the absorbents. He cited the case of a lady who had been under the care of Dr. Henry Davies, and with whom such spontaneous bursting and collapse had taken place. The lady experienced no further ovarian annoyance, and died, ten years afterwards, of another disease. On a post-mortem examination, made by Mr. Brown, it was found that the cyst had completely shrunk, and that an aperture of some size existed in it, probably where the cyst had formerly burst. The preparation is preserved in the museum of St. George's Hospital. Mr. Brown considered the present operation far less dangerous than ovariotomy, and he did not apprehend any mischief from the opening of the peritoneum, as he had made incisions into that membrane in many cases before, for various purposes, without any untoward results. Mr. Brown had had under his care a very old lady, affected with a multilocular ovarian cyst. She was treated in this manner: one of the numerous cysts collapsed, and gave no further uneasiness. This treatment was not, however, pursued, on account of the lady's great age. But it might be expected that more success would be obtained in the present case, where the cyst is probably of the single kind.—*Lancet, March* 13, 1852, *p.* 265.

152.—FIBROUS TUMOUR OF THE LABIUM—ENUCLEATION—RECOVERY.

By Alfred Massey, Esq. Under the care of Dr. Oldham.

[This growth, which was, when first noticed, about the size of a small nut, had, at the expiration of 12 months from this time, attained the size of a small hen's egg. On the cessation of the catamenia, it increased in size much more rapidly, until it so far interfered with her comfort as to cause her to seek medical assistance. Its appearance is thus described:]

The right labium presents an ovoid enlargement, extending from the commencement of the labium externum to its posterior commissure, in length measuring about 5 inches, 3½ to 4 in breadth, and about 2½ inches above the left labium, *which it partially* overlaps and hides. Its surface is perfectly even, the integu-

ments covering it rather tense, and readily and freely movable over the mass; its structure is soft and yielding, and so like a fluid sac, that a grooved needle was passed into it, which was, however, only followed by a little blood.

She was ordered to be kept quiet and at rest. Bowels to be kept open by saline rhubarb powder; having done which, on the fifth day of admission, was operated on by Mr. Alfred Massey at Guy's Hospital, in the presence of Dr. Oldham and Mr. Poland. An incision was made in a longitudinal direction from one extremity of the labium to the other, to the inner side of a large venous trunk, and just within the mucous surface. A finger was then introduced between the integuments and tumour, and its loose connexion with the surrounding tissue forcibly torn through, and the mass completely enucleated. One artery of considerable size was tied, but the further bleeding was entirely venous. A compress of lint was then placed in the wound, and pressure made upon it by means of pledgets of lint and a T bandage. The tumour measured about 4½ inches in length, 3½ in breadth, and 2½ in depth, was invested by a complete covering of cellular tissue but slightly adherent, its surface being perfectly even, and of a pale pinkish white colour; externally its structure was soft, but internally it had a more dense nucleus. *On section* its surface was of a pale pinkish colour, almost white, having the appearance of white fibrous tissue; internally its texture was more dense than externally, and also less vascular. *Under microscope* presented a cellulo-fibrous structure, having numerous cells embedded in a fibrous net-work; no appearance of malignant matter.

The wound has maintained a remarkably healthy appearance, granulations springing up in all directions. Suffering from no constitutional disturbance whatever, the only application used being warm-water dressing, with pressure.

Observations by Dr. Oldham.—Among the tumours which attack the external labia, the fibrous tumour has not been noticed by the more popular and recent writers on the Diseases of Women. It is far more rarely met with than the encysted tumour of the labium, the warty growths, the hypertrophied labium, or even the epithelial cancer of this part. But it is more frequent than tumour which has been called the oozing tumour of the labium, of which not a solitary specimen has occurred for the last fifteen years in Guy's Hospital. The foregoing case affords a favourable example of the locality of the fibrous tumour, its slow growth, the symptoms it produces, its diagnosis and treatment. It is developed in the cellular tissue of the labium, of a round or ovoid form, of little sensibility, gliding easily beneath the integument, which is unaltered in colour: it is in itself sparingly vascular, although, according to the size of the growth, some arteries of varying calibre coil upon its envelope, and then penetrate within it, and return the blood by corresponding veins, both classes of vessels being comparatively unsupported in the surrounding loose cellular tissue. It is slow in its increase, but it enlarges more rapidly during pregnancy, and after the last menstrual crisis. The symptoms it produces are more inconvenient than painful, and increase with its development. When small, it is only discovered accidentally; but, as it grows, pains of a dull, tense, dragging character are felt in the labium, increased by walking exercise; and at last, it may interfere with progression and sexual intercourse. The tumour in this case has been mistaken for a hernia, and it had been subjected to injurious pressure by a truss. In other cases which I have seen, the tumour not being so large, has occupied the central or lower part of the labium, and has been so defined and isolated as at once to prevent any error of this kind. But in the present case it had grown sufficiently upwards to swell out the upper part of the labium, near the external abdominal ring, and to require, therefore, the consideration of its possible hernial origin. On handling it, however, the physical character of the mass was quite unlike either intestine or omentum; it was readily removed from the abdominal ring, which was felt to be quite clear, and coughing had no effect upon it, and there was no difficulty in at once determining that it was a tumour altogether independent of hernia. The fibrous tumour is far more likely to be confounded with the cysts of the labium, and, indeed, their physical characters are often so strikingly alike, that the diagnosis can only be made out by puncture. When very small—about the size of a nut—the cystic tumour is as hard, movable, and in the same position, as the fibrous tumour, and, even in larger growths, the two tumours are closely alike. In the fibrous tumour the tissue is so little consc-

lidated, and is set in so loose a bed of structure, as to give it, when handled and touched with the greatest care and nicety, much more the feeling of a fluid than a solid; and it was on this account that I passed a needle into the tumour in this case to make sure of its fibrous character. The diagnosis is of importance, because the operation for removal, which is the only practical cure in the fibrous tumour, is a tedious process when dissecting out a cyst which may be cured by other less formidable means. My clinical clerk, Mr. Massey, enucleated the tumour, when it had been exposed by a free longitudinal incision, with the fingers, and, in this way, broke through the blood-vessels which supplied it. It is of some practical importance to remember, that these tumours are closely coated with a covering of dense cellular tissue, and it is necessary to cut through this in order to turn out the growth. It is quite easy for the operator to mistake the capsule for the surface of the tumour, and to be trying to separate the integuments from it, which would be a long process, and to prevent this, it is better to cut slightly into the tumour itself, when the edge of the capsular covering is at once indicated, from whence the enucleation is readily completed. The venous bleeding requires to be controlled by a sponge and pad.—*Med. Times and Gazette, March* 6, 1852, *p.* 242.

153.—*Calcification of Fibrous Tumour of the Uterus.*—Mr. I. B. Brown exhibited a specimen of fibrous tumour, which had been transformed by calcification into a solid, heavy body, weighing eight ounces. It was situated at the fundus of the uterus. The sides of the uterus were not adherent to this body, but were distended so as to form a sort of close-fitting bag to the tumour; the neck of the uterus was drawn up and lost in the body, and the os was elongated and thin. This interesting specimen was found in a patient aged seventy, under the care of Mr. Langley, of King-street, Portman-square. The calcareous crust of this tumour consists of semi-transparent plates, overlaying each other, having a glassy fracture; these dissolved in dilute muriatic acid allowed an abundant escape of carbonic acid, leaving a residue of an imperfect fibrous basis substance.—*Med. Times and Gaz., Jan.* 3, 1852, *p.* 23.

154.—ON THE FINAL CAUSE OF MENSTRUATION.

By Dr. Francis H. Ramsbotham.

If it is really the case, of which there seems no reasonable doubt, that at each menstrual period in the human female the fimbriated extremity of one or both of the Fallopian tubes embraces an ovarium, and causes a Graafian vesicle to burst and shed its contents into the canal, it would naturally be inferred, that the formation of this fluid is subservient to the departure of the ovule from its ovarian bed, and designed to perform some important function in relation to its escape. Now, it would appear probable, that that function is identical with the nutrition of the young ovum, and that the menstrual discharge, indeed, is nothing less than the rudiments of the deciduous membrane itself; or rather, that it would have become the deciduous membrane, provided conception had occurred. And I think the identity of these two products is established by the following considerations.

An ovule ripe for impregnation parts from the nest in which it had been elaborated, being conveyed by the grasp of the Fallopian fimbriæ. At the same time, nature establishes an action for the purpose of preserving it, provided an opportunity of becoming impregnated by contact with the male semen is afforded it. Should that contact take place, and conception follow, the fluid formed is retained within the uterus, and is gradually converted into the deciduous membrane, which becomes the first medium of communication between the newly animated ovum and the maternal vessels. If, on the contrary, conception does not happen, the ovule perishes, and the fluid secreted for its advantage, not being required, is allowed to exude externally, as a superabundant and useless excretion.

This supposition, indeed, would require us to believe that the ovule may be impregnated, as well in the Fallopian tube, after its escape from the ovarium, as *in the ovarian bed* itself; and I can discover no difficulty in believing that such

should be the case. In the genus aves, for example, the eggs are impregnated after they have escaped from the ovarium, and in that of pisces the same takes place, not only after they have parted from the ovarium, but even after they have been expelled from the body of the parent altogether; so also in some amphibia, as in frogs.

The variation in regard to the time that elapses between conception and the commencement of labour, observable not only in different women, but also in the same woman on different occasions, may, perhaps, be accounted for by the part of the Fallopian tube at which the ovule becomes impregnated; the nearer to the uterus the ovum was, the shorter the time probably that would elapse before it arrived at the uterine cavity; the nearer to the fimbriæ the longer would be the time, because it would have a larger portion of the Fallopian tube to traverse. The period of utero-gestation, properly so called, that is, the length of time the ovum remains within the uterine cavity, is, in my estimation, definite; while the time of transit through the tube varies considerably, after impregnation has been effected; and this variation will be sufficient to explain the difference above alluded to.

The view which I have taken of this question is strengthened by the facts, that the menstruous fluid and the decidual membrane seem both to be formed by the same tubular glands, lately discovered in the uterine substance,—that the decidua, when first formed, is of the consistence of a viscid fluid,—that in dysmenorrhœa a membrane is not unfrequently formed within the virgin uterus, which has very much the external characters of the decidua, and, indeed, can sometimes scarcely be distinguished from that membrane, the result of impregnation,—that those females who menstruate irregularly or painfully are not so obnoxious to pregnancy as those in whom the function is normally performed,—that the catamenia will sometimes appear once soon after impregnation, as though more fluid had been afforded than was required for the purpose intended,—and especially that, as in the lower animal, no deciduous membrane is formed, therefore there is no necessity for any menstrual secretion; and we know that woman is the only animal subject to this peculiarity.

The position then deducible from the foregoing observations is, not only that whenever impregnation occurs a secretion is elaborated by the uterus for the purpose of affording nourishment to the ovum, but that, independently of conception taking place, an ovule, even in the virgin, passes periodically from the ovarium into the Fallopian tube, and at the same time the same provision is made by nature for its preservation, in anticipation of its becoming vivified; but that if this vivification is not effected, the fluid formed flows away, is cast off, indeed, as effete matter, and is what we popularly call the menstruous discharge.

The periodical return of the discharge cannot be considered as militating against this theory, but rather as supporting it; not only because we have constantly before our eyes instances of functions in the body performed with periodical exactness, but also because we know that in those of our graminivora which bring forth but once annually, the season of conception is so determined that the young should be produced in the spring. Thus the mare and the ass, whose period of gestation is eleven calendar months, conceive almost immediately after parturition,—the mare on the ninth day after, the ass on the seventh,—to the evident intent, that a new progeny may be reared in the summer months. The cow, whose period is nine months, does not, however, conceive till three months after her last birth. The sheep and goat, which carry their young five months, will not take the male till the end of autumn; and the hare, whose term is only thirty days, does not become impregnated until eleven months have passed since the last parturition. This extreme variation in these different races of animals is evidently instituted with one single object, namely, that the young may be produced into the world at that season of the year most favourable for their nurture. And if nature has been so precise in regard to the lower animals, we cannot wonder that she has displayed an adherence to a similar system in the case of the females of our own species, or that there should exist in the human subject the same kind of periodicity in regard to the perfection and escape of the ovule from the ovarium.

Nor can the waste which such a frequent loss of the ovule must entail on nature, be regarded as an objection, since we see, as well in the animal as in the vegetable kingdom, loss to an enormous amount, exactly of the same description, constantly

going on. How few seeds comparatively, even after fecundation, become productive, and what a wholesale destruction of organic life is there not witnessed in the case of fishes. Of the million or the million and a half of ova expelled from the ovarium of the sturgeon, for instance, how few are fecundated, and how few of those that are fecundated survive to be elevated into the existence of a living independent animal. If nature permits such a waste to be inflicted on her, in the reproduction of one genus of animals, we cannot surely find difficulty in believing that the same prodigality (so to speak) may in a minor degree influence her operations in the human subject.—*Med. Times and Gazette, Jan.* 17, 1852, *p.* 57.

155.—*How to make a Sponge Tent.*—By Dr. Edward Rigby.—[Speaking of the best means of dilating the os uteri, Dr. Rigby says]

A well-made sponge tent is a most effectual means for dilating the os uteri to a considerable extent, with but a moderate degree of force, and but trifling pain; indeed, some patients appear to suffer nothing more than a sensation of pressure from it. But a well-made sponge tent is a great desideratum, and I believe that the medical man must make them himself if he wishes to insure them of the requisite quality; for it is very difficult or impossible to get them made so firmly, that they can be introduced without breaking, and yet be capable of expanding to the necessary amount.

A piece of tolerably fine sponge, previously well dried, should be soaked in mistura acaciæ, and rolled up into a cylindrical form, somewhat in the shape of a small cigar, tapering to a point at one end. The other, or thick end, must be rolled round a middling-sized awl, partly for the purpose of leaving a central perforation into which the end of the instrument which carries it is to be inserted, and partly to fix it, while a piece of stout cord is wound tightly and closely round it from the thick end up to the point. By this means, the sponge is powerfully compressed into the cylindrical form above mentioned, and, if well dried, becomes as hard as a piece of wood, and retains its compressed state perfectly when the cord is removed. Any little projections or roughnesses may be trimmed off with a sharp knife; and, lastly, the tent is to be dipped several times in melted tallow rendered harder by the admixture of a little white wax, until it has become thickly coated. A piece of string or tape is fastened to the lower or thicker end to assist in removing it from the os uteri when expanded. The heat of the part soon melts the unctuous covering, and thus enables the tent to slide up in its own grease as it gradually melts, when otherwise it might have been difficult to introduce it. The secretions of the part slowly pervade the sponge, and dissolve the hardened gum with which it has been soaked, and the sponge gradually expands as it returns to its full size.

Twelve hours is usually a sufficient period to effect this in; and the degree of dilatation produced will guide us as to the introduction of a larger tent on the removal of the first.—*Med. Times, Dec.* 6, 1851, *p.* 582.

156.—ON PHYSICAL DYSMENORRHŒA.

By Dr. Henry Bennett.

Dysmenorrhœa may depend, as demonstrated by Dr. Mackintosh of Edinburgh, on a physical imperfection of the uterine neck; on contraction of the os internum, or of the canal which constitutes the cavity of the cervix. This contraction may be either congenital, or the result of inflammation. The peculiar character of the dysmenorrhœa, when caused by congenital contraction, is the absence of *any* uterine symptom during the interval of menstruation, and intense agonizing pain for a few hours before the flow of blood appears, either then disappearing, or lasting throughout the period; these pains commencing with menstruation in early youth. If they are occasioned by inflammation, there are the same symptoms at the time of menstruation, but there is not the same immunity from uterine symptoms in the interval of the catamenia.

The cause of the pain experienced under these circumstances is evident. The

cavity of the non-pregnant healthy uterus not containing more than about ten or eleven drops of fluid, as soon as the catamenial secretion commences from the lining membrane of the uterine cavity, unless the blood find a free exit through the os internum and the cavity of the cervix, it distends the uterus, and gives rise to great pain. The obstruction may merely be at the os internum, spasmodically contracted; in which case, as soon as it has been overcome, the blood escapes freely, and pain disappears. But if the os internum is permanently contracted, or the contraction exists in the cervical canal, the pain may continue throughout the catamenial period.

A contracted state of the upper part of the cervical canal, or of the os internum, is not, I believe, an unfrequent complication of inflammation of the cervix, from the swelling and hypertrophy of the substance of the organ which it occasions. This remark, however, does not apply to the *inflamed region* of the cervical canal, which is uniformly dilated by the existence of inflammation.

I do not, however, think that Dr. Simpson's criterion of the existence of contraction of the os internum is entirely to be depended upon. Dr. Simpson believes, if I am right in my interpretation of his views, that unless the uterine sound pass without effort into the uterine cavity, there is contraction of the os internum. Now the careful examination with the sound of many hundred females has led me, as I have elsewhere explained, to a different conclusion. There evidently exists at the os internum a kind of muscular sphincter formed by a strong band of the circular muscular fibres of the cervix, and destined to close the uterus during the latter stages of pregnancy. Generally speaking, this sphincter, in the natural state, is sufficiently closed to prevent the uterine sound passing into the cavity of the uterus, unless a considerable amount of pressure be exercised. In nearly all the females I examine in the interval of menstruation, the sound passes easily along the cervical cavity, but stops at the os internum; and that when there is no reason whatever to suppose the existence of a morbid coarctation.

It appears to me, on the contrary, as I have elsewhere stated, that a free communication between the cervical and uterine cavities, allowing the *easy* introduction of the uterine sound, is generally an anomalous condition, indicating the existence of disease, unless observed soon after menstruation, when the os internum relaxes, or soon after parturition, when it has not yet had time to recover its normally contracted state. The principal morbid conditions in which I have observed a free communication between the two cavities, are inflammation and uterine tumours. If the inflammation which exists at the os uteri, and in the lower part of the cervical cavity, ascends as far as the os internum, it appears to relax the muscular contractility of that region. The os internum is always open when the inflammation passes into the uterine cavity, and implicates its lining membrane. The same effect is also produced by the development of the uterine cavity, through the formation of tumours in the substance of the uterus, or from any other cause, the os internum gradually opening as the uterus enlarges, probably by the same mechanism as in pregnancy. This is so generally the case, that the fact of the uterine sound penetrating easily through the os internum into an enlarged uterine cavity, may be considered a valuable symptom of the existence of such tumours, to add to those with which we are already acquainted.

Extreme dysmenorrhœa from congenital contraction of the cervical canal and os internum, independent of inflammation, is, I believe, of *rare occurrence*. This is a fortunate circumstance, as it is most embarrassing to treat, requiring an amount of interference with the uterine organs which it is very painful to have to propose to an unmarried female. Dilatation of the contracted cervical is, however, sometimes the only means we have of remedying an amount of suffering at the catamenial period, so extreme as to render life nearly a burden, and as to react deeply on the general health.

Whatever may be the cause of dysmenorrhœa, the mode in which the menstrual secretion takes place is modified by its existence: instead of a flow of bright blood, regular and continuous, although generally increasing by exercise and diminishing by rest, we have a dark, interrupted, clotted discharge. After severe uterine pains which may last many hours, and are often accompanied by tenderness, and swelling in the ovarian regions, and pain in the back and down the thighs, more or less dark clotted blood is thrown out. Its expulsion is generally followed by re-

lief, and by a freer flow for a while, when it again diminishes, and the same ordeal again takes place. Sometimes the interruption will be complete for one, two, or three days, the pains subsiding with the menstrual flux, and returning when it again makes its appearance. The venous condition of the menstrual secretion shows plainly that, either from inflammation, congestion, or some other cause, the uterine circulation is defective, the blood stagnating in the vessels of the uterus, remaining in its cavity, and distending it after it has been secreted.

Treatment.—Constitutional dysmenorrhœa may be palliated in its attacks, and can seldom be removed by medical treatment. A great deal of subsequent uterine disease would, however, be spared to those young females who unfortunately present it, were mothers more generally aware that its existence constitutes throughout life a strong predisposition to uterine inflammation, and that they cannot take too great care of such of their daughters as suffer from it. For such young females the discipline of public schools may be said to be nearly always too severe, and often to lay the foundation for much future physical and mental misery. That this must be the case, will be easily understood when we reflect that the domestic treatment of this form of dysmenorrhœa consists principally in *rest* and *warmth*. Females who suffer habitually from dysmenorrhœa, whatever their age, should remain quietly at home, taking care to preserve themselves from atmospheric vicissitudes during the first day or two of menstruation, which is the period during which the pain is mostly felt. A warm hip-bath will often be found useful. If the pains are very decided, it is even best to confine the sufferer to bed, and to apply warm linseed poultices to the lower abdominal region—a valuable and simple mode of soothing pain.

In mere constitutional dysmenorrhœa, these simple means nearly always suffice to render the pain very bearable. If they do not produce relief, that fact alone constitutes a suspicious circumstance, and should induce the medical attendant to scrutinize narrowly the state of his patient, lest there should be some morbid or physical cause.

In severe dysmenorrhœa, connected with uterine disease, the only *efficacious* treatment is that of the cause of the disease which occasions the dysmenorrhœa. As time is required, however, we are often called upon, even in these cases, to treat the dysmenorrhœa as a symptom: and, warmth and rest failing, recourse must be had to medicinal agents. By far the most efficacious remedy with which I am acquainted, is the injection of laudanum, or any other preparation of opium, into the bowel. From fifteen to thirty minims of laudanum, mixed with a little warm water, should be injected into the rectum, and will generally exercise, if retained, as much influence in soothing the uterine pain as would double the quantity taken by the mouth. Moreover, the nausea and headache which opiates occasion are much less likely to be produced when they are thus administered. If the first opiate injection is not retained, a second, half an hour later, will generally be more successful. I have also found chloroform of great value in these cases. It may be inhaled or administered by the mouth in doses of from twenty to forty minims, mixed with mucilage, the yolk of an egg, or with camphor, which favours its suspension in water. I have given it by injection, but with less success, as it appears, generally speaking, to irritate the rectal mucous membrane, and is consequently not retained. When it is retained, the sedative effect is nearly always effectually produced. Although chloroform may thus often be resorted to with great benefit in dysmenorrhœa, I do not find that as much reliance can be placed on it as in opiates.

There are various other medicinal agents, principally antispasmodics and narcotics, which may be administered with benefit in dysmenorrhœa. We may mention more particularly the various ethers, and especially sulphuric ether, hyoscyamus, belladonna, musk, valerian, and camphor. It must not, however, be forgotten that these remedies are mere temporary palliatives; that dysmenorrhœa when constant and not constitutional, nearly invariably recognizes some physical cause—generally speaking, uterine or ovarian inflammation, and that it is this cause which we must find out, and remove, during the interval of menstruation.

It is the fact of dysmenorrhœa being so frequently caused by inflammatory *disease, that* explains the success which often attends bloodletting, both general *and local, and which* has induced so many authors to recommend it, although un-

aware of the pathological state which it relieves. General bloodletting acts by revulsion; whilst local bloodletting directly relieves the congested and embarrassed abdominal circulation. I seldom, if ever, resort to general bleeding in dysmenorrhœa, because the relief which it gives is obtained at too great a sacrifice of the strength of the patient, and cannot, moreover, be depended upon. A few leeches applied to the groin, or, better still, to the neck of the uterus, when possible, if the discharge is scanty or temporarily arrested, is much more likely to mitigate the pain, and with less loss to the economy. Purgatives, which are frequently useful, act in the same way as leeches, by depleting the abdominal circulation. Some authors, amongst others Dr. Gooch, have considered dysmenorrhœa to be frequently akin to rheumatism, and have recommended colchicum, guaiacum, and other medicines usually given in rheumatic affections. That the uterus may be the seat of such an affection, is undeniable; but I am persuaded that its frequency has been greatly exaggerated, as likewise that of irritable uterus. Indeed these two conditions may be said to have been, to a great extent, mere theoretical creations, destined to account for pathological conditions, the real nature and meaning of which has, until recently, been a mystery to the profession.—*Lancet, Jan.* 17, 1852, *p.* 67.

157.—MORBID VASCULARITY OF THE LINING MEMBRANE OF THE FEMALE URETHRA.

By Dr. George T. Gream, late one of the Medical Officers to Queen Charlotte's Lying-in Hospital.

Under the name of "Vascular Tumour of the Orifice of the Meatus Urinarius," this affection was first described by Sir Charles Clarke, in his valuable work on the 'Diseases of Women;' a work giving evidence of the highly practical knowledge of that author, and which, although published more than thirty-five years ago, may still be considered as one of the best guides to the diagnosis of this class of maladies.

He writes: "There is in most women a degree of projection round the orifice of the meatus urinarius, and from this part sometimes the tumour arises, to which the above name of the vascular tumour of the meatus urinarius has been applied." I have ventured to refer to this affection under another name, because my own experience, confirmed by that of others, tends to show that it does not always appear as a tumour, but that it may be present under other forms, accompanied by the same general as well as local symptoms.

Dr. Ashwell has correctly described the disease, but he speaks of it more especially as a tumour, and states that it is rarely seen after the cessation of the menses. I am led to think that he is mistaken in this respect, for I have witnessed the disease as often in elderly women as in the young.

Dr. Meigs, of Philadelphia, has alluded to the complaint in his volume on 'Females and their Diseases;' but he merely refers to it as a portion of the lining membrane of the urethra, "hypertrophied and inflamed, which may be readily cut off." His observations, however, are contained in a very few lines, in which the importance of the disease is altogether overlooked.

I am not aware that any other authors have noticed the affection at all; but certainly none have regarded it with that consideration which it calls for, when we consider the suffering attending it, and its liability to return, unless properly treated and entirely removed. The fact that it is one of those diseases which do not frequently come under the notice of medical men, renders the circumstance of its being so little alluded to by authors especially remarkable.

It presents itself under three different forms: the first, most likely, being the incipient stage of the second; and the second, the beginning of the third. But this is only conjecture; for the opportunity of proving it has not yet been afforded to me. It may be present as a simple vascularity of the lining membrane of the urethra, without any elevation whatever, extending some little distance towards the bladder; the membrane itself being highly florid in colour, and extremely tender when touched, or during the passage of the urine. This is the usual character of the disease, when it is confined within the canal; but Sir Charles Clarke relates

the case of a patient in St. Bartholomew's Hospital, in whose urethra there was a tumour of a scarlet colour, nearly filling up the canal. The occurrence of a tumour, however, within the urethra is unusual. This is probably owing to the pressure of the sides of the canal preventing the elevation of the dilated vessels, and to the passage of the urine having a similar effect.

When the vascularity is within the urethra, no morbid appearances present themselves externally; but if the symptoms call attention to the part, and the lining membrane is exposed by making pressure around the meatus, the highly florid appearance will at once be detected.

The second form in which the disease appears, is that of a flattened vascular spot, with but slight elevation, surrounding the orifice of the urethra, highly florid in colour, and exquisitely tender when touched; it is so little elevated that it can scarcely be called a tumour. The redness extends from it into the canal for some little distance, but the membrane within, although florid in appearance, is quite smooth on its surface; whereas the external spot of vascularity is slightly granulated, because it is not modified by pressure from the sides of the urethra.

In the third stage, the disease consists of a distinct tumour, granulated, and attached, sometimes by a broader base, sometimes by a narrow one, and, in some instances, even by a slender pedicle to the side of the urethra, or just externally to it; and, in almost all cases, some dilated vessels will be seen extending from its base to within the urethral canal.

When there is an actual prominent tumour, the local pain and the constitutional symptoms are greatly increased in severity. In some cases, the peculiar scarlet colour of the part has attracted the notice of the patient; but in many instances, particularly when the vascularity is within the urethra, not only has the actual seat of the disease escaped her observation, but it has also been overlooked by her medical attendant, who has referred to the uterus as the diseased organ, has stated that its cervix was inflamed or ulcerated, and caustic has sometimes for weeks, or months, been applied, without affording the least advantage to the patient.

This vascular disease is not at all to be considered as similar to an affection situated in the same parts, having its origin in a varicose state of the veins, which causes some uneasiness, and is accompanied by a mucous discharge, but which does not produce the same acute suffering nor the great constitutional disturbance, nor is the appearance the same. In the vascular disease in question the blood contained in the vessels is arterial, while in the venous enlargement it is dark coloured, and the distended veins have the same appearance which veins have in other parts of the body when in a varicose condition. Attention is first called to the vascular disease, by an uneasy sensation at the lower part of the body, and pain passing down the thighs; and pain when urine is voided, or when the part is touched; slight bleeding also occurs occasionally, owing to the rupture of some dilated vessel, whose covering is always much attenuated. There may be frequent desire to pass urine; and walking causes great suffering; while accompanying these symptoms, there is always copious mucous discharge, which is excessive when the disease appears in the form of a tumour. Owing to which, as well as to the constant uneasiness and frequent acute suffering, the patient becomes emaciated and weak, and it is surprising to find so many and such severe symptoms arising from a disease whose extent is confined within such limited bounds; but there is clear evidence that it does produce them in the fact that, immediately upon the destruction of the vascular spot, or even on its partial removal, a comparative freedom from the symptoms is at once enjoyed.

Upon a digital examination of the vagina being made, great tenderness is experienced by the patient at the vaginal orifice, and still more if pressure is made towards the pubes; and bleeding from this part will almost always be induced by the examination. These symptoms necessarily call for further investigation, and the nature of the disease becomes apparent.

The only mode of cure is the destruction of the entire congeries of vessels; and if the smallest part of it is left, the disease will most certainly return. It has been customary to employ excision with scissors, and afterwards to apply potassa fusa, or to use the latter alone for the removal of the complaint. A ligature has also been recommended as a means of removal; but there are objections to both *these modes of* treatment, which those who have used them will, I am confident,

admit. In order that the scissors may be employed, the vagina must be held open by an assistant; but the spot to be excised is so covered by the pubes that it cannot be sufficiently exposed to ensure the due performance of the operation, and the potassa fusa applied to the bleeding surface is effectual only in those cases in which the disease is very superficial, and by itself this caustic always fails to destroy the part entirely; hence we have a return of the complaint, and a repetition of a most painful operation.

If the disease is within the urethra, these means of cure are totally inapplicable. If the ligature is employed, it may in some cases prove efficient; but in others it would be impossible to effect a cure by means of it, as the part affected could not all be enclosed.

Having several times been called upon to treat cases which had been before apparently cured (by myself and others) by the means mentioned above, I was led to think of another mode of treatment which would be more successful: and it occurred to me that the application of strong nitric acid, in the manner adopted by Mr. Henry Lee for the destruction of hæmorrhoids (and which proves so successful), would be equally applicable to the vascularity of the female urethra.

Sufficient time has now elapsed since its application in three instances, and yet there is not the least inclination to a return of the disease in them; and as in others, although more recently treated, there is likewise no such disposition, I am able to speak with some confidence respecting this mode of cure. Its comparative advantages consist in the acid being minutely applicable to each individual part of the affected spot, which it has the power of entirely destroying, whether within the urethra or outside of it; in its producing no fear in the patient as does the anticipation of an operation by a cutting instrument; and in the pain which it causes very quickly subsiding. Dr. Ashwell observes: "The main trouble we encounter in the treatment of these tumours, is their tendency to reappear. If they are snipped off with scissors, and the part allowed spontaneously to heal, there is every probability that it will repullulate and cause the same symptoms. If these growths are not movable and attached by a pedicle, I have found that the diligent application of nitrate of silver freely applied over and around them, will eventually get rid of them; but the process of destruction is tedious and attended *with great agony*. It is generally indeed necessary to apply opium to the part after application, and to soothe the patient by some morphia or extract of hyoscyamus at night." Now there is no such necessity after the application of nitric acid; but the patient complains of no pain after a few minutes have elapsed, and she is able to walk about without inconvenience.

But there is a difficulty in exposing the part sufficiently, and in preventing the sides of the vagina from collapsing too soon after the application of nitric acid; and this is overcome by the use of a speculum, invented, I believe, by Mr. Hilton, for the removal of hæmorrhoidal excrescences. A portion of the side of the speculum, extending nearly to its internal extremity, can be removed after its introduction into the vagina, and if this part of it is just under the pubes, the spot of vascularity will project into the tube; but should only the lining membrane of the urethra be vascular, it will be readily exposed by pressing the speculum firmly towards the pubes against the surrounding part; and the acid can be applied while the pressure is kept up.

A small rod of glass, or a piece of hard wood in the form of the stick of a camel's hair pencil, is the best thing with which to apply the acid; and this should be held to the part for about a minute, care being taken that each enlarged portion of the vessels is completely destroyed, and in about three or four minutes the pain attending it ceases, and the speculum can be removed. It will be better to examine the part in about four days from the time of the application of the acid, and it often will be found healed with no trace of the complaint left. More frequently it presents an unhealed sore, but an absence of the disease. If, however, there be any vessel remaining having the peculiar scarlet colour, it should be again touched with the nitric acid, otherwise the symptoms will rapidly return.—*London Journal of Medicine, Jan.* 1852, *p.* 53.

158.—*Ulcerated Cancer of the Breast; Removal of the Pain by* Dr. James &c.

NOTT's *Freezing Process.* Under the charge of Mr. SHAW, at Middlesex Hospital. —[In this case, such had been the rapidity of the disease, that although the patient was considerably advanced in years, and first symptoms of cancer had appeared only a year previously, yet the surface of the scirrhous tumour was now ulcerated, and at times extremely painful.]

It appears that Dr. James Arnott's plan of producing insensibility, by freezing the part with a mixture of ice and common salt, was tried in this case, in order to allay the agonizing pain which the poor woman was suffering. This trial was attended with very satisfactory results, and the patient was so much relieved by the proceeding, that she soon afterwards requested to have the ice applied again, and she expresses herself as very grateful for the temporary removal of the severe pain she experiences.

Dr. Tyler Smith likewise succeeded, some time ago, at St. Mary's Hospital, in relieving the excruciating pain by the freezing mixture, in a case of cancer of the uterus. It is plain, however, that the apparatus must be somewhat more complicated when the disease has attacked that organ. The fact that the pain accompanying carcinoma of the womb was thus allayed, should certainly not be lost sight of.

Most of our readers probably know, that Dr. James Arnott advises equal quantities of ice and common salt to be mixed together (the former being well pounded), and then placed in a gauze bag, the margins of which are attached to a gutta-percha ring. By gently touching the part to be rendered insensible, with the bottom of the bag, for a minute or two, the surface becomes suddenly frozen, insensibility follows, and the pain of course disappears. To obviate the tingling sensation which is apt to ensue upon the return of sensibility, ice without salt is to be used, and thus no uneasiness whatever is experienced. We have seen portions of the human frame thus frozen, and always noticed that the insensibility became very great.

Dr. Arnott has proposed that his process should, in certain operations, take the place of chloroform; but it is plain that insensibility can hardly be carried deep enough for the generality of operative purposes. Where, however, a thin stratum only is to be implicated, it might certainly be used with advantage. To relieve the pain of cancer, it seems, from the preceding case, and others which have been recorded, to deserve attention at the hands of those who have to prescribe the palliatives which are so indispensable in that melancholy affection.—*Lancet, March* 20, 1852, *p.* 284.

159—*On Hysteria.* By DR. J. M. MACLAGAN.—Mr. Raven details the case of a young woman, who was thrown into strong hysterical convulsions, by seeing the death of a relation. She had been treated by fœtids, volatiles, cathartics, &c., but without relief. She was admitted into hospital under the care of Dr. Alderson, who, having lately seen the good effects of colchicum in severe cases of *chorea*, was induced to try it on her. Thirty drops of the tincture were taken every eight hours. In a few days the convulsions left her, and did not return. In *chorea* also colchicum has been employed with reported benefit. The cases are mentioned of three children having been relieved from this disease in three or four days, by using from ten to twenty minims of the tincture of colchicum daily.—*Monthly Journal of Medical Science, Jan.* 1852, *p.* 28.

ADDENDA.

160.—ON THE FUNCTION OF THE SPLEEN AND OTHER LYMPHATIC GLANDS, AS SECRETORS OF THE BLOOD.

By Prof. BENNETT, Edinburgh.

[In the 'Monthly Journal of Medical Science' for March, Dr. Bennett has a paper on this subject, read before the Royal Society at Edinburgh, in which he treats of]

1, The relation between the colourless and coloured corpuscles of the blood; 2, The origin of the blood corpuscles; 3, Their ultimate destination.

1. *Relations between the Colourless and Coloured Corpuscles.* Dr. Bennett believes, with Mr. Wharton Jones, that the coloured corpuscle is merely the liberated nucleus of the colourless cell. The transformation takes place in the following manner:—The colourless cell may frequently be seen, by the aid of acetic acid, to have a single round nucleus; but more commonly the nucleus is divided, each half having a distinct depression, with a shadowed spot on the centre. Occasionally, before division takes place, the nucleus becomes oval, elongated, and sometimes bent, or of a horse-shoe form. It may be divided into three or four granules. These stages are figured by Dr. Bennett; they were discovered by him in his interesting observations on leucocythemia, and in experiments on mammals, birds, reptiles, and fishes.

He does not believe, with Mr. Wharton Jones, that all the nuclei forming the coloured corpuscles, in mammals, should necessarily be provided with a cell-wall. Many, however, do proceed beyond this point, and may be seen to have cell-walls: the nuclei, in such cases, increase endogenously by fissiparous division, and on the solution of the cell-wall, become coloured blood-discs. In fishes, reptiles, and birds, the coloured blood-corpuscles are nucleated cells, originating in the blood-glands.

2. *Origin of the Blood-Corpuscles.* This (as was enunciated many years ago by Hewson) is to be looked for in the lymphatic glandular system; under which head are included the spleen, thymus, thyroid, supra-renal, pituitary, pineal, and lymphatic glands. Nuclei and nucleated cells are found in these bodies; and Dr. Bennett's observations on leucocythemia have shown that an increase of colourless cells in the blood is connected with the enlargement of the spleen and other glandular organs. The blood of the splenic and portal veins is always richer in colourless corpuscles than that of the systemic circulation; and in young animals, in which the thyroid, thymus, and supra-renal glands are most fully developed, the blood contains most colourless corpuscles. Moreover in a case of enlargement of the thyroid body, this organ contained cells and nuclei of much smaller size than usual; and corresponding cells and nuclei were found in the blood. In another case, the colourless corpuscles in the blood were of two distinct sizes, corresponding with a similar appearance in the corpuscles of the lymphatic glands. It is difficult to determine how the corpuscles find their way from the lymphatic glands into the blood; but Dr. Bennett suspects that there must be a direct venous communication. He believes that, if he has established that the corpuscular elements in the so-called blood-glands are transformed into those of the blood, it will follow that the lymphatic glands secrete the blood-corpuscles in the same way as the testes secrete the spermatozoa, the mammæ the globules of the milk, or the salivary and gastric glands the cells of the saliva and gastric juice.

The most probable and consistent mode of origin of the corpuscles is in an organic fluid, by the production of molecules, the successive developement and aggregation of which constitute the higher formations. Multitudes of free nuclei join the blood, and are at once converted into coloured blood-discs; and the cells circulate for a time, when their walls are dissolved, and their nuclei becomes coloured. The number of coloured corpuscles in the blood increases in proportion to the developement of the lymphatic glandular system in the animal kingdom; and Mr. Drummond and Dr. Bennett have observed that the nuclei in the spleen,

varying in size in different animals, correspond with the nuclei of the blood-corpuscles.

3. *Ultimate Destination of the Blood-Corpuscles.* Dr. Bennett believes that the blood-corpuscles are dissolved, and, with the effete matter absorbed from the tissues of the lymphatics, constitute blood-fibrin. Zimmermann believed that fibrin resulted from the metamorphosis of the textures. The arguments which support this view appear to Dr. Bennett to be unanswerable. There is no fibrin in the chyme; very little in the chyle; less in carnivora than in herbivora. There is no fibrin in the egg; nor in the blood of the fœtus; and very little in the new-born infant. On the other hand, all those circumstances which cause exhaustion of the textures, or increase the amount of absorption, augment the quality of fibrin; as after inflammatory or other exudations, starvation, violent fatigue, pregnancy, and frequent bleeding and hemorrhage. The amount of fibrin in the blood seems out of proportion to what would be required for textural nutrition. Increase of fibrin is also accompanied with diminution of the red corpuscles; hence it appears probable that fibrin results from a solution of the blood-corpuscles, conjoined with the effete matter derived from the secondary digestion of the tissues, which is not converted into albumen.—*London Journal of Medicine, April* 1852, *p.* 366.

161.—ON DISEASES OF THE GLANDS OF THE NECK IN CHILDREN.

By Dr. Tyler Smith.

[Dr. Tyler Smith advances an opinion respecting the origin of these affections which we think ought to be had in memory during their treatment. He says:]

The lymphatic vessels take their rise, among other tissues, from the skin and mucous membrane, but particularly from the latter. Different tracts of mucous membrane even have their different congeries of lymphatic vessels and glands, with which they are in intimate relation; as, for instance, the bronchial membrane and the bronchial glands, the urethra and the inguinal glands, the vagina and rectum, and the glands of the pelvis. In prolonged bronchial irritation we get enlargement of the bronchial glands. In urethral inflammation we have enlargement and suppuration of the inguinal glands; in irritation of the rectal and vaginal mucous surfaces, fistula or pelvic abscess from glandular inflammation and suppuration. Certain parts of the tegumentary surface are also in special relation with certain groups of glands, as, for instance, the skin of the upper extremity with the glands of the axilla, and the skin of the inferior extremity with the glands of the femoral region. It is quite unnecessary to do more than refer to the frequency with which injuries of the skin are followed by glandular suppuration in these situations.

Now, I am convinced by careful observation, that the mucous membrane of the mouth, nares, and fauces, is in special relation with the great number of glands and lymphatic vessels studding every part of the subcutaneous surface of the neck, and I am equally convinced that in the great majority of cases of enlarged cervical glands, the source of the disease exists in the neighbouring mucous membrane. Some irritation is present in the mucous surface, which affects the lymphatic vessels, and these propagate it to the glands, the glands inflame, pus is formed, the skin gives way, and the unhappy child is said to be the subject of a scrofulous sore. The scar is indelible, and ever afterwards the suspicion of scrofula is preserved.

We see the influence of irritation of the mouth and fauces in producing enlargement of the cervical glands in its most marked degree in scarlatinous and other forms of sore throat. We see it also in the irritation of the mucous membrane in measles, dentition, salivation, aphthæ, carious teeth, canker, disease of the gums, enlarged tonsils, common catarrh, a thickened state of the nostrils and Schneiderian membrane, thrush, and other affections to which this important division of the mucous membranes is subject. When there is a depraved state of the constitution, scrofula breaks out in different organs simultaneously; but when we see a local origin producing strictly local disturbance, and spreading only from point to *point around the first* disorder, I submit that we may fairly give the first place to

local causes. In the end, no doubt, the persistence of local disorder may, and often does, produce the scrofulous constitution. The direction in which local disease of the lymphatics extends is generally from the surface of the body towards the thoracic duct, the centre of the lymphatic system; but this is not invariable; disease may travel down reversely from the lymphatics of the axilla or inguinal region to the knee or shoulder joints, but its extension in these directions takes place very slowly.

It is in states of debility that inflammation of the lymphatic system most frequently occurs. In this, as in many other points, the lymphatics resemble the veins. The tendency to inflammation in the lymphatics may be produced by the very means taken to subdue inflammation in organs of higher vitality; it is frequently seen in its most marked form after bleeding and mercurialization. In the chronic form, it travels so slowly, that its extension as a local malady is completely masked. Although the primary irritation is so frequently derived from the mucous surface, when a gland has once inflamed, it becomes a source of irritation to other glands in its vicinity, which in turn become inflamed, particularly if the primary gland suppurates.—*Lancet, May* 15, 1852, *p.* 464.

162.—ON THE DETECTION AND PRESERVATION OF CRYSTALLINE DEPOSITS IN URINE.

By Dr. Arthur Hassall.

The *materials* required for the preservation of the deposits which occur in the renal fluid when in a morbid condition, are, test tubes, distilled water, a camel's-hair brush, glass slides, cells, covers for the cells, and cements.

The *slides*, for the sake of uniformity, and the convenience of disposal in the drawers of the cabinet, are now all made of one size, viz., three inches long by one broad; in general the slides are quite plain, but sometimes they are hollowed out in the centre into little pits or cells.

The *cells*, when separate, should be made out of thin glass, and should be furnished with a large aperture; they are to be affixed exactly in the centre of the slide by means of the cement, to be described hereafter.

The *covers* are to be of still thinner glass, of a circular form, and are to be accurately fitted by means of cement over the aperture in the cells.

Three different *cements* are required to secure the cells and covers; one should consist of marine glue; the second of asphalte dissolved in turpentine; and the third of a mixture in equal parts of the second cement and gold size. The marine glue is to be heated over the flame of a candle, and used to fasten the cells to the slides; with the asphalte the margins of the cell are to be coated over, and on to this, when nearly dry, the glass cover is to be dropped. The object of applying this coating is, that by means of it the cover may be so secured as to prevent the third cement, which is used to secure the edges of the cover, from running into the cell, and so spoiling the preparation. The slides, cells, and covers may all be procured from the principal manufacturers of microscopes.

Next to urate of ammonia, deposits of *uric acid* are the most frequent of all; they may be recognised by the eye alone, by their fawn colour more or less deep, and sandy or crystalline texture. Sometimes, however, the sediment is so pale, although not absolutely colourless, and the crystals so minute, that a microscope is necessary to determine their nature; and by the aid of this instrument they may at once be recognised. The primary form of the uric-acid crystal is a rhomb which is met with in every modification of size and shape, and in one of two states—either the crystals are single and separate, or else compound and aggregated, glomeruli or spherules being formed by the crossing at angles and union of several crystals. It happens occasionally, when the crystals are very small, that some of them, in place of falling to the bottom of the vessel, form a delicate scum, or pellicle, on the surface of the liquid, which is very apt to be overlooked altogether, and the nature of which the microscope only can make known to us.

Deposits of uric acid may be permanently preserved in the following manner: the supernatant fluid is to be poured off, and the sediment transferred to a test-

tube: this is to be filled with distilled water, which, as soon as the deposit has entirely subsided, is also to be carefully carried away, or removed by means of a pipette; the tube is then to be filled a second, and even a third time, with distilled water.

The object of washing the crystals is to get rid of the mucus and soluble salts, which, as the liquid evaporated, would collect round, and render the crystals indistinct.

But sometimes the crystals of uric acid are concealed in a dense cloud-like sediment of urate of ammonia, and this must be got rid of before the uric acid can be obtained separately. The separation may be thus effected: the supernatant liquid, except so much of it as contains the urate, is to be poured off; the remainder is to be transferred to a large test-tube; this being gently heated over a spirit-lamp, the urate is re-dissolved, and the uric acid left free and unaffected; this is then to be washed in distilled water, as above described.

The deposit being now well washed, and freed from adherent mucus and the soluble salts of the renal secretion, is to be removed from the test-tube, and placed in one of the cells previously affixed to the slide. A soft camel's-hair brush will be found to facilitate the transference, and to assist in distributing the crystals evenly over the interior of the cell.

The cell is next to be set aside until the crystals have become perfectly dry; lastly, the cover is to be put on and secured as directed with the appropriate cements.

Now precisely the same steps must be taken in order to preserve all the other deposits enumerated; they must all be collected in the same manner, and well washed in distilled water.

Urate of ammonia is the commonest of all the deposits; it occurs generally as an amorphous powder, very soluble in hot, and but sparingly so in cold water; occasionally, but rarely, it is met with in the form of little shot-like spherules, and when in this state only is it possible to preserve this salt in a satisfactory manner. The deposit should be but once washed in very cold distilled water. Sometimes the spherules collect on the surface, or adhere as a brown crust to the sides of the bottle containing the secretion.

Uric acid and the urates of ammonia and soda are the only *coloured* deposits which occur in the renal secretion, with the exception of that very rare deposit, cystine; the rest are all colourless, and herein we have a simple character by which uric acid and its combinations may be distinguished from the other deposits.

The *neutral phosphate of ammonia, and magnesia* or *triple phosphate*, as it is sometimes termed, is also very frequently present as a deposit; the crystals have the form of a three-sided prism, and are often so large that they are visible to the naked eye. In a white glass bottle held up in the rays of the sun, the crystals of this salt, as they descend in the fluid, are to be seen shining like myriads of minute diamonds.

Very commonly, some of the crystals, in place of falling as a sediment, float on the surface of the liquid, forming a pellicle; sometimes the crystals are free, but more usually they are imbedded and entangled in a gelatinous-looking layer of phosphate of lime.

The modifications in the form and size of the crystals of the triple phosphate are very great, but the whole of the varieties are reducible to the one primary form.

The bibasic triple phosphate is a rare variety of renal deposit, and is met with in the fluid of the kidney, principally when this has been kept for some time. It presents itself in the form of beautiful feathery stellæ, which are extremely difficult to preserve in a satisfactory manner, as, in drying, they are apt to fall to pieces, and so become disintegrated.

Another rare form is the *phosphate-of-lime deposit.* The crystals occur sometimes singly, at others in tufts or stellæ. Although they usually fall as a sediment, yet they may often be detected floating on the surface of the liquid, either free or intermixed with crystals of triple phosphate, or entangled in a pellicle formed of vibriones.

Oxalate of lime is of rather frequent occurrence as a deposit. It may occur alone, but more commonly it is mixed up with either triple phosphate, uric acid, or urate

of ammonia. Some writers state that the crystals frequently do not form a sediment, but remain suspended in the liquid, and direct that a portion of the renal fluid should be gently warmed in a watch-glass to allow of their subsidence. This is unnecessary, as, if sufficient time be given, the crystals always fall and collect at the bottom of the vessel containing the secretion. If, as is frequently the case, there be much epithelium or mucus, the crystals will become entangled, and concealed indeed, in these to some extent. The dumb-bell form of oxalate of lime is not so uncommon as is generally supposed. As in the case of the other compounds and salts, the crystals sometimes collect on the surface of the liquid. I have met with crystals of oxalate of lime, both octohedra and dumb-bells, so small that their form could scarcely be distinguished even with an object-glass magnifying 420 diameters.

The last of the deposits met with in the renal fluid is *cystine*. This is exceedingly rare; it forms a copious pale-coloured precipitate, resembling to the eye that of urate of ammonia, but immediately distinguishable under the microscope by its crystalline texture, the crystals forming flat, six-sided lamellæ, which are often compound.

Many of the *soluble* compounds and salts likewise admit of being preserved, in a manner more or less satisfactory, as urea, nitrate and oxalate of urea, the chlorides, sulphates, and phosphates. Into the method of preserving these I propose to enter in a second communication.

I have at the present time in my cabinet several hundred preparations of the above deposits, put up in the manner just described, and many of which have been thus preserved for years. Of the more interesting and curious varieties I have had figures executed, and am now engaged in the preparation of these for publication. —*Lancet, May* 15, 1852, *p.* 466.

163.—ON CHROMIC ACID AS AN ESCHAROTIC.

By Dr. Fromer.

Chromic acid has been, on the recommendation of Dr. Heller, tried in many cases as an escharotic, in the syphilitic wards and clinique of Professor Sigmund; the results hitherto obtained would justify a further trial of it in cases in which a deeply penetrating, gradual caustic, and one constant in its effect, would be indicated. When employed in substance its action is exceedingly slow and gradual, occupying many hours; nevertheless in intensity it exceeds that of the caustic alkalies. In extremely concentrated solution (to insure saturation, a slight excess of crystals should be allowed to remain undissolved in the fluid) its action is less penetrating and less gradual; but, at the same time, it is more continuous than that of all other known caustics; on the other hand, the more dilute the solution, the more transient and superficial is the effect. The facility with which its action can be thus graduated renders it in all cases a suitable escharotic.

In order to test its action, the first experiments were performed on new growths and marks; viz., on pointed and broad condylomata, occupying the genitals and inner surface of the thighs in different individuals, and varying in thickness from two to eight lines; and in their longest diameter from half an inch to two and a half inches.

Modes of Application.—The surrounding parts having been protected by folds of lint, strips of adhesive plaster, &c., the chromic acid in substance (mixed with sufficient water to form a paste) is spread with a spatula on the part to be cauterized, so as to form a layer scarcely a line in thickness, which is covered with lint, kept in its place by adhesive plaster. The concentrated solution is applied by means of a glass rod, a pencil of asbestos, or, if necessary, a hair pencil (which, if immediately washed, can be employed again); it is left exposed for a few minutes, and then covered with dry lint.

Objective and Subjective Phenomena.—Chromic acid in solution communicates its colour unaltered to the parts moistened with it; after a few minutes the colour becomes gradually darker, changing from light brown to dark brown. The parts to which it has been applied remain moist, bright, and glossy, for from fifteen to twenty minutes; the acid then gradually dries, and changes the surface it rests on.

to a dark brown, partly bluish black, dry elevation, which slowly becomes thicker, and after forty-eight hours either falls off spontaneously, about one or two lines in thickness, or can easily be separated in its entire extent. The ulcerated surface left by the scab is generally covered with a firmly attached greyish-white exudation, which in from twenty-four to forty-eight hours gives place to clean granulations, of which the cutis forms the substratum. The scab formed after the application of undissolved chromic acid is thicker, adheres longer (from five to six days), and the loss of substance is very much more considerable. The inflammatory symptoms in the surrounding parts are very trifling, the redness is scarcely two or three lines in width, and there is little or no swelling.

If chromic acid be applied to sound parts, a moderate sensation of burning commences in ten or fifteen minutes after the application, increasing for three or four hours, and then diminishing for about an equal space of time. Its application to ulcerated or excoriated parts excites similar sensations instantaneously; these likewise increase for four or five hours, seldom remain stationary, and again diminish during an equal period.

Undissolved chromic acid causes severer and more permanent pains; these are also more violent when the cutis vera itself, and not morbid growths, is cauterized, and for this reason each succeeding cauterization of such growths is attended with more pain than the preceding one. The pain does not disturb the patient's sleep, is incomparably less than that caused by other caustics, such as sulphuric or nitric acid, nitrate of silver, nitrate of mercury, corrosive sublimate, caustic potash, Vienna paste, &c. The solid chromic acid, on account of its penetrating action, should be employed but seldom, and then with extreme care; and for the same reason the concentrated solution, which has been found sufficient to destroy condylomata of seven lines in height in six applications, is to be preferred.

According to the experiments of Dr. Heller, all organic compounds are soluble in the easily deoxidizable chromic acid, their ultimate elements being raised to a higher degree of oxidation, and partly uniting with the acid. An elevated temperature accelerates this process. Smaller animals (mice, birds, &c.) were so completely dissolved by chromic acid within fifteen or twenty minutes, that no trace of their bones, skin, hair, claws, or teeth could be discovered.

Chromic acid would thus appear to be not only a gentle and gradual escharotic, but also a complete and rapid solvent.—*Wiener Med. Wochenschrift.*—*Dublin Quarterly Journal of Med. Science, Feb.* 1852, *p.* 250.

164.—*Iodine rendered Soluble by Syrup of Orange-peel and Tannin.*—M. Debauque mentions in the 'Journal de Pharmacie' of Antwerp, that he has found means of keeping iodine in a state of solution, when added to mixtures in the form of tincture. The author uses, for that purpose, syrup of orange-peel, which answers the purpose perfectly. It was suspected that *tannin* was mainly instrumental in this result; and this was rendered evident by putting a few grains of tannin into a quantity of water to which tincture of iodine had been added, and in which the iodine had of course been precipitated. The addition of the tannin caused the iodine to be immediately re-dissolved. Thus will the syrup of orange-peel be advantageously added to mixtures containing tincture of iodine, and tannin to injections composed of water and the same tincture.—*Lancet, Dec.* 13, 1851, *p.* 556.

165.—ON THE ENDOSMOTIC ACTION OF MEDICINES.

By Dr. Cogswell.

At the London Medical Society, Dr. Cogswell, after remarking on the construction of the apparatus—the properties of different membranes—the well known deductions of Poiseuille, in his Memoir in the 'Comptes Rendus' of the French Academy of Sciences in 1844, proceeded to make the following observations:]

The endosmometic of Dutrochet consisted of a glass tube, with a somewhat *bell-shaped moveable* expansion called the reservoir, having a deep contraction

round the middle for securing the membrane. The form of reservoir preferred by Dr. Cogswell was that of a bell-jar with a projecting rim round the larger orifice, the end of the tube and the inside of the reservoir being ground to fit one another. The reservoir had a capacity of eighteen drachms, and an internal diameter at the larger orifice of an eighth of an inch. The calibre of the tube was a fourteenth of an inch. To support the reservoir the tube was passed through a cork adapted to a hole in a leaden plate, which rested on the edge of the outer vessel. This was a glass cylinder, of such dimensions, that, on receiving the reservoir, a quantity of fluid, equal to the contents of the latter, would rise to the neck, leaving sufficient below the membrane. On consideration, the author had been led to adopt, for closing the reservoir, the cœcum of the sheep, as sold in a prepared state by the French, finding, in comparison with other membranes, that it produced the most marked results. The experiments of Poiseuille were then examined in the order observed in his Memoir :—

Action of Purgatives.—Seidlitz water contained in the reservoir, being opposed to serum, ascended in the tube. Albumen was found in the reservoir, and sulphate of magnesia in the serum. Now, seidlitz water causes an unusual quantity of albumen to appear in the alvine discharges, and of sulphate of magnesia in the urine. Hence the inference is, that this class of purgatives possesses the property of determining a flow of serum towards the bowels. The author remarked, that it might reasonably be questioned whether serum was a fair representative of the living fluid in the blood-vessels, or its accumulation in the bowels the only physiological effect of the saline purgatives.

Tolerance of Medicines.—The author remarked that endosmose was found by Poiseuille to stop at periods varying for different fluids. The outer fluid being then examined, presents a striated appearance from the incomplete diffusion of the foreign matter introduced into it. After shaking it, there is a renewed ascent of the column; and the same thing happens repeatedly. Poiseuille employed a solution of phosphate of soda and serum. The author repeated the experiment with a solution of the salt, of density 1060, and obtained similar alternations, except as regards the elevation following the second employment of the serum. He left it to be judged, whether the facts as stated would bear out the inference, that the tolerance of medicines arises simply from the circumstance, that "the membranes of the intestinal canal, after being long in contact with the same substance, become impregnated with it, and prevent it from entering so freely into the circulation."

Influence of Opium.—Opium and its salts check diarrhœa, and obviate the purgative tendency of other medicines. A solution of one part of nitre to eight of water was opposed by Poiseuille to serum, and produced an elevation in the tube for three quarters of an hour. While the endosmose was proceeding vigorously, the solution was withdrawn, and replaced by a similar one, containing muriate of morphia. After this the ascent continued, but with less intensity; it proceeded for an hour, ceased an hour, and then the column began to descend. Hence, it is said, the presence of the morphia diminished the endosmose, then put a stop to it, and ended by producing exosmose, such being precisely its effects in promoting constipation of the bowels. The author, however, believed, that if the experiment had been continued without the morphia, the result would have been nearly the same, as he had found that nitre by itself has but a feeble power of endosmose. To ascertain further, whether opium exerts a peculiar influence on membranes unfavourable to endosmose, he had repeatedly opposed an aqueous solution to water, and found it produce much greater effects than some of the inorganic salts. The serum of the sheep inclosed in a reservoir, and opposed to distilled water, containing a grain to the ounce of muriate of morphia, produced a vigorous endosmose for above twenty-four hours. Added to syrup in the same proportion, its effect was not appreciable. He was thence led to believe there was not sufficient ground for characterising morphia as a substance, the presence of which puts a stop to endosmose, and renders the membrane impermeable to either fluid.

Influence of Tobacco.—The decoction of tobacco is stated, by M. Poiseuille, to penetrate the membrane, and render it unfit for endosmose. A decoction of four parts of tobacco-leaves to forty of distilled water was opposed to serum. There was a descent of the column in the tube. However, the density of the two fluids was not stated. The author having made a similar decoction, found that, after

boiling above an hour, the density did not exceed 1023, when it was not likely to produce endosmose with serum having a density of probably not less than 1026. But a decoction of this strength, being opposed to distilled water, produced an elevation lasting for several hours; and further, a decoction of density 1052 opposed to serum of density 1031, produced a well-marked elevation of the column, which was found not to have stopped in twenty-one hours. The author proceeded to state, that having observed a great variety in the endosmose afforded by different solutions of the same density, he tried the following experiment:—Four endosmometics, closed with the prepared cœcum, were filled respectively with solutions of sugar, sulphate of magnesia, common salt, and nitrate of potash, and placed in distilled water. In half an hour the first fluid ascended 1-9th inch, the second 1 inch, the third 2 inches, and the fourth 1-8th of an inch. Other membranes afforded corresponding, though less marked results. Thus the common salt was the most energetic at first, and the nitre the least so. But again, the syrup and sulphate of magnesia continued to ascend for several hours, while the common salt stopped in four hours, and the nitre in less than two. Syrup, though it has a remarkable power of endosmose, is not a purgative, which Poiseuille accounts for by its being decomposed by the gastric juice. The author then extended the examination to classes of substances. The results obtained were arranged in a tabular form, and laid before the Society. It was remarkable, that the sulphates from which experience had selected the most generally useful purgatives, had invariably a strong and continued action; while the class to which nitre belonged was comparatively feeble. Chlorate of potash and the iodide and bromide of potassium were among the substances which had the lowest place in the tables. Gum and liquorice showed a moderate degree of energy, but it continued uninterruptedly for weeks. The author, after entering into some further details, said, he mentioned these as coincidences, which might prove useful aids to investigation, but without any view to the premature construction of a theory. From what preceded, he was led to the following conclusions:—

1. That the division of substances into those which are favourable to endosmose, and those which on the one hand retard and annihilate it, by their influence on the membrane, and on the other render the membrane permeable, or reduce it to the condition of a filter, requires confirmation.

2. That the power of endosmose of different solutions is not regulated entirely by their density, as already observed by Dutrochet.

3. That the purgative salts generally have an energetic power of endosmose, and that this is exerted with more steadiness and uniformity by those which medical experience has selected as the most useful in ordinary circumstances.

4. That some of the other substances have marked peculiarities with regard to endosmose, which will probably assist towards explaining the mode of action on the system.

Dr. Lankester spoke in terms of commendation of Dr. Cogswell's original and interesting paper. It might, possibly, not be considered practical; but many of the theories referred to by the author resulted in practical uses. Investigations proving the errors of old theories were as important as those by which new ones were established. The investigation of the physical properties of matter contributed much to our knowledge of the functions of life; and, although our knowledge of those properties of membranes called exosmosis and endosmosis was very imperfect, it had nevertheless opened out a field for useful inquiry. Dr. Cogswell's experiments proved the theory explaining the action of saline purgatives, by their increasing endosmosis, to be only partly true. The action of other medicines might, perhaps, be explained by them. The great endosmotic power possessed by acetate of ammonia was very remarkable; it showed that this power was not the sole cause of purgation, and might also explain the action of that medicine. He was rather astonished at the conclusions with respect to morphia, as the experiments of Poiseuille and Bachetti showed that it lessened and even reversed the endosmotic action of fluids in which it was dissolved. This explanation of its action in diarrhœa was, consequently, rendered of no use. It must be recollected, however, when reasoning from phenomena occurring out of the body with reference to those which take *place within it*, that the conditions were different. In the human stomach and *intestines there is a* living surface covered with cells in a constant state of deve-

lopment, and also with mucus, by which any endosmotic action must necessarily be modified. If Dr. Cogswell's paper only led to negative results, it would still be serviceable, as indicating the necessity for caution on a subject on which there had been a great deal of positive speculation.

Dr. HANDFIELD JONES remarked, that certain simple homogeneous membranes possessed the power of altering the nature of fluids that passed through them; and adduced the instance of the Malpighian tufts of the kidney. This was a circumstance of some interest to consider in reference to endosmotic action. He then alluded to the case of the renal secretion, in which the blood containing the elements of the secretion was on one side of the homogeneous basement membrane, and a layer of albuminous semi-solid matter, in the form of epithelium, on the other; and he suggested, that the elimination of the secretion might be an act of endosmosis.

Dr. SNOW said, that although endosmosis was a very important subject for consideration, it did not assist much the explanation of the action of medicines, even of those of the purgative class. It might sometimes aid the action of some of the saline purgatives, such as Epsom salts; but that drug would purge when repeatedly given in small doses, so diluted as to be of much less density than the serum of the blood. One important point necessary to be borne in mind with respect to endosmosis, has been mentioned by Dr. Golding Bird,—viz., that acetate of potash and other salts, when prescribed as diuretics, must be diluted to such an extent as to enable them to be absorbed, otherwise they would induce endosmosis in the alimentary canal, and act as cathartics. Opium, probably, arrested purgation by lessening the peristaltic action of the intestines. The theory, that it diminishes the permeability of animal membranes, would not explain its power of arresting diarrhœa, even if it were correct; for the absorption of fluids taken into the alimentary canal would be retarded, which would exert a contrary effect. In order to fully understand the action of medicines, other laws must be considered as well as those governing endosmosis.

Mr. CHIPPENDALE said that much praise was due to Dr. Cogswell for the manner in which he had conducted his observations, and brought them before the Society. Still he thought if their object was to show that the operation of inorganic salts, as purgatives, is effected by a process of endosmosis, the author had failed. For, in the first place, the fluid found in the dejections is not serum. Secondly, if this were a transudation of fluid by endosmosis, we should expect this to take place principally through the coats of the stomach, and to be gradually diminished along the alimentary canal. Yet experience taught us that the operation of purgative salts is principally in the colon. Again, if serum were to pass through the coats of the alimentary canal by endosmosis, this would be continually going on; forasmuch as the mucus which lubricates the inner surface of the tube is more dense than the serum. If a glaring instance were required to demonstrate that the action of purgatives was not one of mere endosmosis of serum, he would adduce what takes place upon the exhibition of a dose of castor oil. He thought, then, that we should look to some other kind of action of these salts, and that this must be one of the epithelial cells.—*Med. Times and Gazette, Jan.* 3, 1852, *p.* 23.

166.—M. JUNOD'S *Exhausting or Hæmospasic Apparatus.*—M. Junod, of Paris, exhibited an apparatus, contrived by him some time since, with a view of causing a vacuum over certain parts of the body, and thus acting as a derivant of the blood, —in fact, as an enormous cupping-glass. The power thus obtained is applied by him to the treatment of inflammation; the good effects of the abstraction of blood being procured without the usual debilitating results which follow the absolute loss of that fluid. M. Junod stated, that by means of this apparatus, four pounds of blood could be drawn into one leg only, the effect being doubled if both extremities be operated on simultaneously. The practitioner thus becomes the master of the circulation; he can reduce the pulse to a very small volume in a few moments, and can augment or diminish its frequency at will, without causing any immediate or ulterior inconvenience. The blood thus displaced, requires twenty-four hours in order for it to re-enter the general circulation, without re-action. One of the most important effects of this considerable derivation of the circulating mass-

um, is an abundant perspiration; with that object in view, all that is necessary is to cause the first degree of syncope; the forehead is soon covered with perspiration, which speedily becomes general, if the patient be placed in favourable condition.—*Med. Times and Gazette, Jan.* 10, 1852, *p.* 50.

167.—*Extract of Bullock's Blood in the Anæmia of Infants.*—Dr. MAUTHNER, physician to the St. Ann's Hospital at Vienna, has lately proposed the above extract, he being convinced that there are many cases of anæmia in the young where steel cannot be given. The extract is prepared in the following manner:—The fresh blood of the ox is first strained, and then exposed in the water-bath to complete dryness. The powder is given dissolved in water, in doses varying from ten to eighty grains, and the best results have been obtained by Dr. Mauthner, after repeated trials. He considers that children are benefited by this remedy, as the latter supplies the very elements the little patients are wanting; viz. fibrine and hæmatosin. The preparation is especially fit for cases of anæmia depending on chronic diarrhœa, typhus fever, pneumonia, on extensive and long continued suppuration, and on serous accumulation after scarlatina.—*Lancet, Dec.* 20, 1851, *p.* 579.

168.—ON THE INTERNAL USE OF ATROPINE.

By Dr. LUSANNA.

[On account of the dangerous nature of this drug, it has not been much employed in England as an internal medicine. In Italy, however, Dr. Lusanna has employed it in certain nervous affections. The most severe poisonous accidents have arisen from its employment even in doses of 1-10th of a grain. The commencing dose is stated as 1-30th of a grain every three or four hours; though in some cases one-third of a grain has been given five times a day. The following cases illustrate its effects.]

Case I. *Epilepsy of Fourteen Years' Duration—Atropine given Internally—great improvement.* The patient was a woman, thirty-five years old, of nervous temperament, leading a religious and ascetic life. After severe mental excitement, at the age of nineteen or twenty, she had epileptic attacks; and these had continued at times for about fourteen years. They had latterly occurred three or four times a day for five or six days in succession. Her aspect was imbecile, and she had hysterical and neuralgic symptoms. On March 23, 1850, I dissolved half a grain of atropine in a little alcohol, and directed it to be taken by drops in water during the next forty-eight hours. In twenty-four hours, the sight was affected, but the pupil was not dilated. On the evening of the 25th, when she had finished the half grain, she was unable to recognise individuals. On this day there had been more threatenings of an epileptic attack, confined, however, to some convulsive tremblings, which rapidly ceased. On March 26, half a grain was again prescribed, to be finished on the 29th. The results were, disturbed vision, unsteady gait, dryness in the tongue and throat, buzzing in the ears, loss of memory, and incoherence in speech. The hysterical headache, and pain which she had felt for some years in the right hypochondrium, had left her. The medicine was interrupted: on the 30th and 31st, the narcotic effects were gradually dissipated: the pupil only oscillated slightly under the influence of light. On April 2, the atropine was repeated in the dose of half a grain for two days; the same phenomena occurred. On the 8th, the dose was increased to one grain: the results were soon marked, and there was a little fever, for which an ounce of cream of tartar was ordered. On the 16th, a grain and a half was ordered. The patient kept her bed, in consequence of vertigo, unsteady gait, and disturbance of ideas and of vision. The iris was fixed; there was dysphagia and dryness of the throat and mouth. She took half a scruple altogether; and, from the 17th to 22nd April, she took three grains in five days. Two days after, the pupil began to dilate; and, on the third day, the *iris had disappeared.* When the medicine had been left off, the iris contracted

gradually, and by successive oscillations: the hypochondriac and neuralgic pains did not return.

On June 18th, there had been no epileptic fits for three months; but the patient had had some hysterical and neuralgic symptoms. This patient still bore on her countenance the peculiar imprint of epilepsy. After seven and a half months of uninterrupted health, she had fresh attacks, in consequence of the threats of one of her neighbours.

Case II. *Neuralgia of the Maxillary Branches of the Fifth Pair of Nerves on the Right Side—Endermic Application of Atropine—rapid cure.* The patient was a woman, aged 35. A blister was applied beneath the right ear, and the surface dressed with an ointment containing one-sixth of a grain of atropine twice daily. In two days the cure was almost complete; but as there were still some symptoms of pain, the quantity was doubled, and the pains completely ceased.

Case III. *Epilepsy since Infancy—Internal Administration of Atropine—great improvement.* On April 18th, 1850, a man, 47 years of age, was brought into hospital in an apoplectiform state. He had been epileptic from childhood; he had, at least three times a month, and sometimes from three to five times in a day, violent attacks, with general convulsions and loss of consciousness. The disease, and his habitual drunkenness, had left deep traces in his external aspect and moral condition. Two days before, he had three or four epileptic fits in the day, after which he had fallen into an apoplectiform state, which still continued, and from which he was with much difficulty roused by bleeding and purgatives.

This was certainly not a favourable case for the administration of atropine: but I resolved to employ it. On May 6, I prescribed half a grain in sixty drops of alcohol, to be taken by drops in two days. In the evening, the pupil was much dilated, and the iris appeared only as a small black circle. The next morning, the pupil was less dilated, but immoveable: the patient was buried in the bed-clothes, but, by calling to and shaking him, he was roused to sit up in the bed. He was a little more reserved than usual. In the evening the medicine was finished; the patient had eaten very little; he kept himself up with difficulty, and walked with a sort of convulsive agitation. He was loquacious, delirious, and incoherent; there was no fever. The atropine was repeated. On the night of May 8th, he had hallucinations and pleasing delirium. The medicine was suspended: but he had delirium and visions all the day. The next morning he had some appetite, the pupil was reduced to two-thirds of its size. The medicine was repeated. On May 13, the dose of the medicine was increased to a grain for two days. It was now better borne; and, except disturbance of vision, and dryness of the tongue and throat, there were no morbid symptoms. On the 18th, the dose was raised to a grain and a half for two days; on the 25th, to two grains; on the 31st, to two grains and a half; on June 6th, to three grains. In short, from May 6th to June 13th, the patient consumed thirty-four grains of atropine. In the latter days, the delirium had increased, and was almost continuous. Up to this date, that is, for two months,—there had been no epileptic fits, when, on the 16th June, while the patient was in the court, he tottered and fell; there were no cries, convulsions, nor foaming at the mouth: the limbs were quite flexible. In less than a quarter of an hour consciousness had returned. This was a very mild attack compared with those which had preceded; I however repeated the atropine on June 21, in the dose of two grains for two days. The appetite was immediately lost. The pupil was fixed; there was mild delirium, and semiparalytic trembling of the extremities. On the 26th and 30th, the same dose was repeated. The trembling was calmed; there was a little appetite and diarrhœa. On the night of the 30th, the patient, having perhaps taken too large a dose, was delirious in the night, and passed his urine and fæces involuntarily. On July 2, the atropine was again given, in the dose of a grain for two days, dissolved in acetic acid and distilled water. On the 4th, two grains were given; on the 6th and 8th, two and a half grains. On the 10th, the medicine was again suspended. On August 24th, there had been no fresh epileptic attack. On September 24, the patient, who had indulged in the use of strong liquors since he left the hospital, had a fit. The atropine was repeated; and, on November 16, the attacks had not returned.

Case IV. *Chorea with Mania—Internal Administration of Atropine—rapid cure.* A man, aged 40, a farmer, was seized, after maniacal symptoms, with chorea.

movements. I prescribed half a grain of atropine in sixty centigrammes of alcohol, to be taken in two days. The narcotic effects were slight, and the movements immediately became free and regular; but the chorea re-appeared in two days. The patient took a grain of atropine on the 6th and 7th, and another on the 8th and 9th. The convulsive movements daily became less; and, towards the middle of June, had entirely disappeared. But the mania continued.

Case V. *Tertian Fever Successfully Treated by the Internal Administration of Atropine.* A young husbandman entered the hospital on July 25th, 1850, for an obstinate tertian fever, which had returned twice, after having been treated with disulphate of quinine. The fever had continued three weeks. On the 26th, after the attack, the patient commenced to take a mixture, consisting of half a grain of atropine dissolved in a little acetic acid and 250 grammes of distilled water. From the evening to the next day, the patient took about 1-24th of a grain of atropine. There was only weight in the head, dilatation of the iris, and dryness of the tongue and throat. In the evening, there was dilated pupil; diplopia; an appearance of mist round objects, which appeared four times their size. The patient could not rise, from unsteadiness of gait: there was dryness of the tongue and mouth; the aspect was tranquil; the pulse 55; the respiration 20: there was a little embarrassment in speech. On the morning of the 28th, the solution had been taken. The pupil was strongly dilated and immoveable; the face and mind were tranquil; there was loquacity; the pulse was 55; there was heat of the tongue and throat. The attack came on two hours later. On the 30th, in the afternoon, there was a slight shivering of the limbs, the body preserving its normal heat: this was succeeded by general heat, with perspiration. These were the last traces of the disease. On April 18, he had to be bled for some symptoms of cerebral congestion.

Case VI. *Tertian Fever of Two Months' Duration—Paludal Cachexia—Atropine given Internally—Cure.* The patient was a female peasant, aged 39 years. On July 9th, after an attack, a grain of atropine was ordered to be dissolved in acetic acid and sixty grammes of water, to be taken by teaspoonfuls in two days. After 1-16th of a grain, there was disturbed vision, dry mouth, and trembling of the limbs. After the second spoonful, she had maniacal delirium, and sopor; the pupil was dilated. The attack appeared on the 11th, three hours earlier than usual. The atropine was carefully repeated on the 12th; the effects were much more easily borne. On the 13th, the fever again appeared. A purgative was now given, and some leeches applied to the anus, for gastro-hepatic symptoms. The paroxysms did not occur for five days. On August 2nd, half a grain of atropine was dissolved in a little acetic acid and eight ounces of water, and given in doses of three teaspoonfuls daily. The disease was immediately arrested without any other treatment; and the patient left the hospital quite well on the 25th of August.

Case VII. *Chronic Vingelitis—Severe Pains—Atropine given Internally—Relief.* A woman of seventy years had chronic disease of the spinal cord, paraplegia, anæsthesia of the whole body, except the head, and severe pains in the lower limbs. On May 31st, half a grain of atropine was ordered to be taken in forty-eight hours. The result was an immediate calm. The half grain was made to last a week; but there were well-marked physiological phenomena, and even delirium, which gradually ceased. On June 18th, the dose was repeated; the patient seemed more used to the medicine and obtained very great relief. The dose was gradually raised to a quarter of a grain at a time. The latter dose produced an erythematous eruption of the skin, with elevation of its temperature.

Case VIII. *Epilepsy dependent on Uterine Derangement—Atropine given Internally, without success.* The patient was a girl aged 22. She took twelve grains of atropine in five weeks, with some camphor. No therapeutic results followed, though the physiological effects were well marked.

Mode of Administration. Until we have soluble salts of atropine, as acetate, nitrate, tartrate, &c., it can only be given dissolved in alcohol, in acetic acid, or some other mild acid. The solution, especially the alcoholic, has a taste resembling quinine; but it is feeble, and not disagreeable. Neither the tincture nor the acetic solution produce any disturbance of the digestive functions: the medicine is readily absorbed. Pills and powders are unadvisable, from the difficulty of exactly dividing the atropine, and the risk of its accumulating in the system. It is first given *in doses of 1-30th* of a grain, gradually increased, the effects being very carefully

watched. The tolerance of the medicine is so great, that a quarter of a grain may at last be given every four hours. For external application in neuralgic affections, Dr. Lusanna applies from 1-14th to 1-16th of a grain, in a pommade, to a blistered surface.

Physiological Effects. 1. *Dilatation and Immobility of the Pupil.* Between fourteen and twenty minutes after taking from 1-24th to 1-30th of a grain of atropine, the pupil is enormously dilated. If the patient be not examined until several doses have been taken, and the medicine has had time to take more effect on the nervous system, this phenomenon is not observed; but there is complete immobility in the iris, in its natural state of dilatation. If the use of the atropine is suspended, the dilatation is reproduced as its effects seem to go off. As the pupil recovers its mobility, it begins to contract by slight oscillations under the influence of a strong light, and to dilate in the shade. The dilatation of the pupil is the last phenomenon to disappear; it has been met with eight days and more after the suspension of the medicine; the return of the mobility indicates that the toxical effects are disappearing.

2. *Disturbance of Vision.* Objects at first seem to swim in a whitish vapour; their *contour* is not neatly defined; persons are not recognised; it is impossible to read or write. If the dose be increased, a thick dark shade is thrown over objects, and the sight may be even lost. Each fresh dose of atropine has a sudden and marked effect in diminishing vision; and the disturbance of sight disappears with equal rapidity. A day or two after the cessation of the medicine, there are no traces of this effect.

3. *Disturbance of the Intellect.* At first there is a languor and slowness of intellect; the individual appears distracted and astonished; his ideas and answers are slow and without judgment. At a later period, the patient has vertigo and confusion of ideas, and is as if half-intoxicated. In one case, there was disturbed intellect and headache for a fortnight, when the other symptoms had disappeared. But this was perhaps not due to the atropine; for it had appeared during his attacks of intermittent fever. The troubled intellect and vertigo was in no case accompanied by any sensation of weight in the head, such as accompanied cerebral hyperæmia.

4. *Hallucinations of Hearing.* These phenomena are more rare than those just mentioned. Whistling and hissing in the ears occurred in an epileptic female; another patient heard tinkling; and one woman, with disease of the spinal cord, had a continual buzzing. These noises very probably become hallucinations during delirium.

5. *Hallucinations of Vision.* These accompany the impairment of sight, and are very variable. They consist of visions of well-known individuals, assuming strange and monstrous forms; of extraordinary and gigantic phantoms; of numerous black insects flying or creeping round the patient; the ground, and all on it, seems to turn round; objects are seen double, surrounded with a sort of cloud, or are only multiplied or enormously enlarged. In short, the hallucinations are of the ridiculous or frightful kind.

6. *Anæsthesia.* This effect is much more perceptible with regard to the pains which the patient may have suffered, than with regard to tactile impressions. These are preserved; except, however, there has been but little disposition on the part of the patient to allow himself to be influenced by painful tactile impressions. This is confirmatory of what some physiologists have written relative to the distinction between the sense of touch and the impression of pain.

7. *Dryness of the Mouth and Throat.* A very few days after the first doses of atropine have been taken, there is a sensation of extreme dryness in the mouth, tongue, and throat. This is a constant phenomenon, and seems purely nervous, as there is no real dryness; at a later period, however, the parts become dry. This effect seems to depend on diminution of the salivary secretion, and to especially correspond to semi-paralysis of the muscles of the pharynx. There is no trace of gastric irritation.

8. *Loss of Appetite.* Although the patient may have had a good appetite, he loses it, and at last is disgusted with food. There is no thirst. When the treatment is suspended, the appetite returns, sharper than ever, almost amounting to voracity.

9. *Embarrassment of Speech.* As the action of the atropine becomes more

marked, there is more or less embarrassment of speech, consisting of difficulty, slowness, and uncertainty in the articulation of words. M. Bouchardat has also noticed this effect.

10. *Delirium and Stupor.* Delirium, alternating with or succeeded by stupor, is produced by one-tenth of a grain at the commencement of the treatment, by one-fourth of a grain at a later period, or by any sudden increase above the usual dose. The delirium is constantly gay and ridiculous; the patient forgets all that is around him, and his imagination is transported to distant and imaginary objects. In only one case was the delirium mournful. When these phenomena have reached a certain degree of intensity, they are very slow in disappearing. For several days after the medicine has been left off, there is confusion of intellect, and a certain incapacity of volition and thought.

11. *Dysphagia.* This is an unfailing result, if the use of atropine be continued for some time. After having experienced dryness of the mouth and throat, the patient finds that deglutition becomes gradually more difficult. In vain he puts the voluntary muscles of the neck into action: the constrictors of the pharynx remain almost immoveable.

12. *Redness of the Skin.* In one case, the skin, which was very fine and delicate, was coloured of a lively red a quarter of an hour after the atropine was taken. The patient was not delirious; the pulse in the arteries was large, but slower than usual. The redness was like that produced by the sun, or rather like that which appears on the ears, hands, and face, on going from a cold place into a warm one. It followed each administration of the medicine, and lasted from half an hour to an hour. I have not met with it in any other case: but M. Bouchardat relates that, in one case, the cheeks of the patient became red after the external use of atropine.

13. *Torpor and Paralytic Tremblings.* When the dose is augmented, the lower limbs become heavy, lose their strength, and fail in supporting the body, or only become weak and tottering: the patient is obliged to remain in bed. From time to time there are transitory tremblings in individual muscles; but no cramps or spasmodic and painful contractions. Some voluntary control can still be exercised for a time: and, when this is lost, there are still subsultus or convulsions. The latter phenomena occur when a large dose has been early given, so as to rapidly bring about loss of volition and consciousness; the former, when the muscular irritability has been gradually diminished, by the doses having been gradually increased.

14. *Paralysis of the Sphincters of the Rectum and Bladder.* This manifests the highest degree to which the medicative action of atropine can attain. In one case, on two separate occasions, involuntary diarrhœa followed the administration of a grain and a half a day; and, on slightly increasing the dose, the fæces and urine passed involuntarily.

The functions of respiration, circulation, and calorification, seemed seldom or never affected by atropine.

I have never observed truly alarming results to arise from the use of atropine. However, as this has occurred to others, I think it right to say that I have, with M. Bouchardat, been led to regard wine as the best antidote. As often as I gave wine to the epileptic patient mentioned in Case 3, the delirium was soon calmed; it also sometimes aided deglutition in this case. In Case 8, also, the phenomena were evidently calmed under the use of wine.

Most of the physiological phenomena produced by atropine result from its action on the cerebro-spinal centres. In all probability, the encephalon first, and most directly, feels its influence; especially its parts corresponding to the senses of sight and hearing, to speech, and to thought. Then are affected the spinal system, the radiating muscles of the iris, the constrictors of the pharynx, the sphincters, and the tenuity and irritability of the general muscular system.

Therapeutic Effects. The cases which have hitherto seemed most favourable for the use of atropine are centric or cerebral epilepsy, neuralgia, chorea, and intermittent fevers. Illustrations of these have already been given. In eccentric, reflex, or symptomatic epilepsy, resulting from disease of some distant organ, atro*pine has failed.* (See Case 8.) *L'Union Médicale*, July 1, 3, and 5, 1851, as

quoted from the *Gazetta Medica Lombarda.—London Journal of Medicine, Jan.* 1, 1852, *p.* 74.

169.—ON THE THERAPEUTIC PROPERTIES OF CREASOTE.

By Dr. Benjamin W. Richardson.

[Since the attention of the profession was drawn to the value of creasote in cholera by Mr. Spinks, of Warrington, Dr. Richardson has given the remedy a fair trial in the treatment of ordinary diarrhœa, and as far as his own observation goes, there is no astringent remedy half so useful in certain cases of profuse purging. Dr. R. says:]

The cases in which I have found creasote most valuable are of three kinds:—

1st. Cases where purging appears, and cannot be traced to the presence of foreign matters in the intestines, as so often happens during ordinary epidemics.

2ndly. Cases where a diarrhœa follows the administration of purgative medicines given for the purpose of removing foreign matters from the intestines.

3rdly. Cases where after an acute diarrhœa, a state of passive purging continues,—the patient being constantly troubled with sudden small liquid evacuations, not attended with great pain, or any considerable constitutional disturbance.

From the first-named class of cases I could give numerous instances illustrative of the good effects of the remedy in question. During the late diarrhœal epidemic many of these cases have come before me; bnt, to save time, I shall only state that, in no case of the kind, have I seen the remedy fail, and that, in most instances, from two to four doses of the medicine, have been sufficient to check the evacuations.

Of the second variety I have also seen several examples. The following case affords a good illustration:—In the month of August, 1850, I was one morning hastily summoned to Ghiswick to visit an aged lady, who was suffering from diarrhœa. I found that on the previous day she had been eating heartily of some kind of indigestible food, and feeling uneasy sensations in consequence, she had taken a smart dose of some aperient medicine. The effect was a profuse action of the bowels, continuing for many hours, and reducing her to a deplorable degree. At the time of my visit she presented the following symptoms: the diarrhœa was constant, the matter evacuated being thin, and but little coloured. The pulse at the wrist was nearly imperceptible, the surface of the body was cold, the lips were blue, the abdominal muscles were painfully contracted, and the voice was greatly diminished in force. I prescribed creasote two minims, with half a drachm of sulphuric ether, to be given immediately in a little syrup, and to be repeated every one or two hours. A little brandy and water was also recommended at intervals, and light mucilaginous drinks. The body to be kept warm. On calling again in the evening of the same day, I was delighted to find my patient much better; she had rallied considerably after the first dose of creasote and ether; the diarrhœa had diminished, and by this time had ceased altogether, two more doses having been given. I now stopped the medicine, and the diarrhœa did not return; for a day or two some slight symptoms of thirst appeared, but these passed off, together with the prostration consequent on so severe an illness, and in the course of three weeks perfect recovery had taken place.

Of the third class of cases I have not seen so many examples as of those previously named, but the following is one of the kind:—A gentleman who was travelling was seized with an ordinary attack of diarrhœa, for which he took medicine with much relief: still his recovery was not complete. He did not suffer from pain, but three or four times in the course of the day he would be suddenly troubled with a small liquid evacuation. This was a great annoyance, and he was treated for it in various ways, by mild purgatives, and also by all the ordinary astringents, but with no avail. He returned to town and resumed his usual pursuits, but the purging, notwithstanding the most careful diet, continued to tease him. Creasote was at last prescribed, in a minim and a half dose, combined simply with syrup of tolu, and a little water. The effect was most satisfactory. The remedy was taken but twice, when the disagreeable symptoms entirely passed off.

Ten days after they slightly returned: but one repetition of the dose named above effectually checked a relapse, and since then (nine months ago), the bowels have acted quite normally.

The advantages which creasote seems to possess over the other astringents, are:—1st. That it succeeds when others fail. Of this fact I am quite convinced: I have, during the late months, when diarrhœa has been so common, selected at the same time similar cases, and whilst treating one with creasote I have treated the others with opium, chalk, or some similar remedy. In such trials I have unexceptionally found the purging arrested much more readily and effectually by the creasote, and in many cases, other remedies having signally failed, this one has been prescribed afterwards with the best results. 2dly. It is very speedy in its action. 3dly,—and this is a great advantage,—it rarely leaves the bowels constipated. In the course of its administration I have sometimes seen it produce symptoms of dryness of the mouth, with a white filmy state of the tongue, and a little excitement. Under such circumstances the remedy is better discontinued; indeed it is not often wanted after these symptoms, inasmuch as the diarrhœa is usually checked before the appearance of them.

With children the remedy is also useful; bnt, to secure success with them, the dose must be very small,—the one-fourth, one-sixth, or one-eighth of a drop to children under two years, is sufficient. With adults from one to two drops is the dose I usually prescribe, and this may be repeated every two or three hours for several times if required.

I am happy to see that the value of creasote as a remedy for diarrhœa has not escaped the notice of other observers. In the last volume of the 'London Medical Gazette,' there is a paper by Mr. Kesteven on this subject, in which he praises the remedy greatly. Mr. Kesteven also offers the theory, that the good effects of creasote in diarrhœa may arise from its power in coagulating albuminous solutions. There is, however, a great objection to this theory, in the fact that to coagulate albumen a large quantity of creasote is required; so that the small doses medicinally administered cannot be supposed as sufficient to produce such coagulation in the intestines.

I must just add one word with reference to some of the other therapeutic properties of creasote. In elementary treatises on therapeutics, I find ascribed to it, narcotic, sedative, and diuretic properties. To none of these effects can I myself bear witness. I have never seen the least approach to narcotism or diuresis on any occasion, from the doses named above; while, on the circulation, I am quite sure it acts rather as a stimulant than as a sedative. Its power to arrest vomiting depends upon the dose: given in a full dose, two drops, I have seen it even bring on vomiting; in which case a few minims of dilute hydrocyanic acid are usefully combined with it. At the same time, I may observe that I have seen diaphoresis follow its administration, and that it possesses very excellent antispasmodic properties. I have also found it very useful in arresting hiccup in cases of exhaustion. To disguise the nauseous qualities of creasote, no vehicle seems to me to answer so well as the syrup of tolu, to which compound tincture of cardamoms may be added. It is also very advantageously combined with opium, or with the preparations of ether, in cases where these remedies are indicated.

In concluding this notice of some of the properties of creasote, I have only to observe, that I do not speak of its effects from a small number of cases, or a short period of trial.

I have tested the remedy impartially for many months.—*Medical Gazette, Nov.* 28, 1851, *p.* 925.

170.—*Galvanic Battery and Belt.*—[Before the Medical Society of London, Mr. PIGOTT exhibited his battery and belt, and also explained their action.]

He said, that in all cases in which it was desirable to apply galvanism, the great desideratum has been to procure a continuity of the galvanic current of low tension or quantity, as distinguished from intensity. This could not be obtained by any of the ordinary acid batteries, the galvanism obtained from them being intense, and *therefore* inappropriate for curative purposes. In the application of Mr. Pigott's *battery, the* current of low tension is easily converted into intensity at the will of

the operator; the battery is, so to speak, a self-acting one, and requires no previous preparation before using. It presents the appearance of a square box of polished mahogany, and varies in size from six to twelve inches square. Mr. Pigott stated that his battery was constructed with the ordinary metals, which were permanently separated by a porous diaphragm, separated with deliquescent salts, which latter, absorbing the moisture of the atmosphere, excited an electric current always ready to be called into action. The galvanic belt is made on the same principle, and when used emits a continuous current of electricity, which passing through the patient, stimulates the nervous system in a highly beneficial manner. The belt is called into action as soon as it is applied, and may be worn without needing saturation, and in fact without the slightest moisture being necessary. Mr. Pigott also mentioned that in several cases of contracted limbs he had used the galvanic battery in the first instance, and then applied the belt, made in the shape of knee and leg bands, with complete success. He had also successfully applied it to cases of neuralgia, dyspepsia, and disorders arising from an inactive state of the nervous system. The belt can be so constructed as to be applicable to any part of the body —viz., wrists, arms, limbs, neck, &c.—*Lancet, Dec.* 27, 1851, *p.* 610.

171.—*Generation of Sugar in a Debilitated Condition*—M. Alvaro Reynoso lately maintained, before the Academy of Sciences at Paris, that there exists a connexion between the phenomena of respiration, and the presence of sugar in the urine. He therefore considers that all the substances which retard the act of breathing, and thus diminish the amount of red blood in the lung, may be looked upon as causing sugar to be generated, and thrown off by the urine. According, then, to M. Reynoso's views, we ought to find sugar in the urine of persons who are taking lowering remedies. To test the truth of this position the author examined the urine of persons taking bichloride, iodide, or sulphuret of mercury, antimonial salts, opium, or sulphate of quinine, and discovered sugar in that secretion. He intends making experiments in order to discover in how much time after these remedies are left off, the sugar disappears from the urine; and whether the absence of the sugar and the complete discontinuance of the remedy are coincident circumstances.—*Lancet, Dec.* 27, 1852, *p.* 604.

172.—CASE OF POISONING BY LAUDANUM IN INFANCY.

By Dr. William Herapath.

[The age of the child in this case was 39 days. Having a slight cough, the mother administered at one p. m., a teaspoonful of laudanum in mistake for cough mixture. At five p. m., all the symptoms of poisoning by opium had evinced themselves. A mustard plaster was immediately applied to the spine, and a strong infusion of coffee and ammonia administered. The child was not allowed to remain still a moment.]

As it was clear that all the poison must have already passed into the circulation, it was useless to give an emetic; if administered, it would have probably failed; and if it had succeeded, it would only have caused the ejection of the antidotes—namely, the coffee and ammonia: and as it was evident that the treatment of the case would last many hours, if successful, but would fail if at all slackened for an instant, I determined upon bringing electricity to our aid. Whilst arranging the battery, my pupils were directed to administer electric shocks from a small Leyden jar, along the neck to the diaphragm; but the child did not appear to respond to this stimulus in the slightest degree. Having now got Horne, Thornthwaite, and Wood's little maintaining and multiplying battery ready, I began, at half-past six p. m., to pass a constant stream of electricity through the little patient.

After numerous trials, it was found that, by placing the zinc or positive wire on the mucous membrane of the mouth, and the negative or copper wire just below the ensiform process, the respiratory movements were carried on with considerably more regularity and ease than by any other method. It was evident that the stream

of electricity, entering by the fifth nerve, was conducted by it to that portion of the spinal marrow (the medulla oblongata) which presides over the function of respiration, and which, being stimulated to action, its influence was sent through the phrenic and external respiratory and spinal nerves to the diaphragm, the intercostal and other accessory muscles of the respiratory process.

The fifth nerve is known to be a natural afferent or excitor nerve of the respiratory movements. We only, therefore, called its physiological action into play, and it cheerfully obeyed the call. Respiration proceeded steadily and regularly as long as the stream continued to be applied in this manner; but if the zinc or positive wire slipped from the cheek to the tongue, the movements became more gasping and convulsive; spasm of the glottis appeared to ensue, and if it had been persevered in, possibly asphyxia would have resulted; as another nervous arc was probably then employed, the gustatory branch of the fifth appearing to act as an excitor (through the medulla oblongata) of the superior laryngeal, thus closing the glottis, and probably inducing an act of deglutition at the same time.

The stream of electricity was maintained, with only an occasional intermission, during several hours. As long as the battery kept in action, all went well; the child breathed steadily, regularly, and almost as if nothing were the matter. At times the stream would get weaker, in consequence of more acid being required, or from the vibrating spring for making and breaking the contact getting out of order; at these times the little patient would experience a serious relapse. At eleven p. m. it had a very narrow escape; the only sign of life left was derived from auscultation; the heart was still found to beat, "tap, tap," about thirty times in a minute, faintly and just audibly. The battery at this nick of time was restored to action, and the aspect of the case again improved. From this period till two a. m. it was constantly maintained in action. At this period the electricity was slightly discontinued, as consciousness began to manifest itself; the little resuscitated patient appeared restless, uneasy, and in pain from the repeated sinapisms; it even attempted a feeble cry. In about ten minutes there were symptoms threatening relapse. The battery was again used for a few minutes, and a stimulating enema of one drachm of spirits of turpentine, six drachms of castor-oil, in about four ounces of strong infusion of coffee, was used, whilst some coffee, with a little brandy, was administered by the mouth. This was swallowed with difficulty; some of it got into the larynx, and produced a fit of dyspnœa, but there was not sufficient nervous energy to induce a cough. Half-past three a. m.; he was so much recovered that he no longer required the stimulus of the battery; he had continued to breathe with tolerable ease during the past half-hour without its assistance. I left it at four a. m. in charge of my pupil, Mr. Phelps, who watched it carefully until I came down, at eight a. m. During the whole of this time the battery had been employed three times for short periods only; the last application was at seven a. m. during ten minutes. I found it breathing naturally and with tolerable regularity; it would occasionally seem to catch in its breathing, as if a slight spasm of the glottis occurred; but by blowing in its face or shaking it, the inspiratory act would be induced, and all would again go on smoothly for some time, when a repetition of the spasm would call for similar treatment.

At one p.m., March 6th, it was sufficiently recovered to be left in the charge of its anxious parents, and to be removed from my house, where it had been all night. It was, however, still highly comatose, with the respiration occasionally stertorous. The bowels had not acted; some little of the enema had returned at the time of its administration, otherwise the greater part had been retained. At four p.m. I visited it: all was going on well; the child appeared to be slowly rallying from its stupor; it could not yet take the breast, but it swallowed small quantities of milk-and-water given by the spoon with more ease; respiration was going on better; the spasms continued to recur, though at longer intervals, and to become slighter in their intensity. The skin began to assume a more natural, healthy tint; it had lost the deadly, sallow hue it formerly had. The temperature of the surface was more natural; there was not that necessity for artificial heat which existed previously, it could now dispense with the fire and the blanket, and was lying in its cot; the sleep was deep, but the breathing had lost its stertor. Eight p.m.: the little patient continued to do well, but had not yet been able to take the breast; I *advised them to* watch it well during the night; in fact, to remain up with it.

• 7th. One p.m.; everything progressing very favourably; the child had awakened from its lethargy; it had taken the breast and cried lustily during the night. All danger therefore appeared gone. A dose of castor-oil was ordered, and I prescribed a slight antimonial mixture for the cough, which appeared to trouble it.

8th. Improving in all respects; ceased attendance.

[Having observed, in 'The Lancet,' the account of the death of Mr. Lloyd's patient from chloroform, Dr. Herapath states the precautions which he has followed with invariable success in its administration. He says]

My rules are—1st. Never to administer it to any patient with a full stomach. 2ndly. To take particular care that the vapour is diluted with plenty of atmospheric air; and I invariably employ a cupped sponge, with large holes, applying but a small quantity of the liquid at one time. 3rdly. Never to proceed to stertorous respiration, if it can be avoided. As soon as the patient becomes insensible to sound, or the pupils dilate, to intermit the application of the chloroform, and operate at once, if necessary applying the sponge again and again, for short periods, in order to keep up the anæsthetic effect. It is never necessary to make the patient lie like a breathing log, as I have frequently known to occur in the practice of other parties. Death from chloroform takes place in consequence of the density of the air mixed with the heavy vapour of the chloroform being nearly equal to or greater than that of the carbonic acid to be expelled from the blood: thus, the density of carbonic acid gas at 60° is one and a half that of atmospheric air; consequently, oxygen and nitrogen endosmose through the membranes of the pulmonary air-cells and capillaries, whilst carbonic acid exosmoses through the same membranes, according to the known laws regulating endosmosis of gases—namely, "in inverse proportions to the square roots of their densities." Now, increase the density of the inspired air by adding the vapour of chloroform to it, and of course carbonic acid accumulates in the circulating fluids; it does not exosmose through the membranes at the same rate. The nearer the two densities correspond, the less carbonic acid escapes from the blood, and the function of respiration no longer goes on. An inspection of the accompanying table of the specific gravities of the following gases and vapours may contribute to the perfect comprehension of this argument.

	Specific Gravity.
Atmospheric air at mean temperature and pressure	1·000
Carbonic acid	1·523
Nitrous oxide (laughing gas)	1·520
Ethereal vapour	2·586
Chloroform vapour	4·200

Now, it follows that it is possible to respire a mixture of one volume of the vapour of ether, if it be diluted with two volumes of atmospheric air, the specific gravity of the mixture being 1·517, for $\frac{(1{\cdot}000 \times 2) + (1 \times 2{\cdot}586)}{3} = 1{\cdot}517$, and one volume of the vapour of chloroform, to be at all respirable, must be diluted with more than five volumes of atmospheric air, as the specific gravity of such a mixture would be 1·533 higher than carbonic acid, for $\frac{(1{\cdot}000 \times 5) + (1 \times 4{\cdot}2)}{6} =$ 1·5333, whilst nitrous oxide, having a specific gravity of 1·520, may be respired perfectly pure during a certain time, after which, the access of oxygen gas is absolutely necessary to keep up vitality.

There is another circumstance worthy of attention. Atmospheric air at 60°, saturated with the vapour of chloroform, has a specific gravity of 1·355, and is therefore perfectly respirable; but increase the temperature of the air to 70°, and it will take up a much larger quantity of the vapour, by which means the specific gravity will be increased to 1·533. It is, therefore, necessary to pay attention to this point also, or danger will ensue. This is more especially requisite where inhalers are employed; a good cupped sponge, moistened with cold water, has numerous advantages over every variety of inhaler I have seen, the chief being, in my opinion, that the rapid evaporation of the chloroform diminishes the temperature of the air as it passes through the interstices of the sponge, thus decreasing the specific gravity of the mixed gases for respiration, and consequently adding to the safety of the patient.

There are other causes why chloroform impedes and destroys the power of life. The local anæsthetic effect of the vapour upon the ramusculi of the pneumogastric nerves, distributed on the air-cells, destroys their excitability; they no longer carry that excito-motory power to the medulla oblongata, so necessary to produce the mechanical movements of the respiratory phenomena; consequently, the motor nerves no longer stimulate the respiratory muscles to action. Again, the blood, highly charged with chloroform and carbonic acid, and deficient in oxygen, may act centrically on the medulla oblongata, destroying its nervous energy and paralyzing its functions. The most practical difficulty is that which the difference in density occasions. We have that fairly under control. The physiological difficulties may also be met by not giving the Lethatic fluid too rapidly and in too great a quantity. The respiration of the vapour of chloroform may be kept up for a considerable length of time provided it be not suffered to accumulate in the air-cells of the lungs, or in the circulating fluid, the occasional respiration of atmospheric air being permitted from time to time.

In resuscitating from an over-dose of chloroform, galvanism is the only chance. Keep up a current of electricity through the fifth nerve, medulla oblongata, phrenic nerves, and diaphragm, as long as respiratory movements can be produced, and let the patient have plenty of fresh air or oxygen gas, and the case must do well, for the blood will remain fluid for a long time, and circulation will go on as long as respiration continues to be carried on artificially. The blood and the air-cells throw off their load, and in proportion as the pneumogastric, medulla oblongata, and motor nerves, slowly resume their functions, so respiration begins to assume a less artificial character; at length the cerebrum aids us, and respiratory movements, both voluntary and involuntary, keep up the functions of life unaided.

It is my opinion, that if the stream of electricity had been employed in Mr. Lloyd's case, in the manner in which I used it with Mrs. B——'s infant—namely, by applying the positive electrode to the mucous membrane of the mouth, and the negative over the diaphragm, and so stimulating the branches and trunk of the fifth nerve, and sending the current to it by the medulla oblongata, and taking advantage of its physiological excito-motory function to induce motor phenomena through the medulla oblongata, phrenic, intercostal, and external respiratory nerves, and thus compelling the whole respiratory system to enter into a combined effort to maintain the function in its natural and physiological action,—it would have terminated as successfully as my own, provided the artificial respiration had been kept up long enough to unload the system of the toxic and Lethiferous vapour oppressing it as the opium did in my patient.—*Lancet, March* 27, 1852, *p.* 303.

173.—Dr. Ogier Ward's *single-tubed Feeding Bottle.*—This biberon differs from all others hitherto constructed, in having only one orifice, through which the air passes into the bottle from the mouth of the child at the same time that the contents are withdrawn by suction only. This effect is owing to the cork mouth-piece being sufficiently firm to resist the atmospheric pressure, which causes other mouth-pieces, of leather or India-rubber, to collapse like a valve, and to oppose the entrance of air into the bottle. Hence it presents the advantage of not leaking in any position, and may be used by any infant able to feed itself, without fear of its wetting; thus obviating the necessity of a nurse to hold it while in use. The cork mouth-piece has the further advantage of having no disagreeable taste or smell, and is firm enough to resist a bite. The whole instrument is of a convenient shape and size, is easily cleaned, and is inexpensive.—*Med. Times and Gazette, Feb.* 28, 1852, *p.* 224.

174.—INHALATION OF CHLOROFORM NEARLY FATAL IN AN OPERATION UPON THE CHEEK AND LOWER JAW.

By Edward Stanley, Esq., F.R.S.

In this case, chloroform being administered by the ordinary inhaler, not the *slightest effect was* produced, and a piece of lint was then folded in the form of a

cone, and the chloroform being dropped upon it, the whole was covered with a folded towel and laid on the face. No sooner had the operation commenced than the most alarming symptoms manifested themselves, the patient being apparently quite lifeless. Cold-air, stimulants, and other means were used to restore consciousness; but it was only after a perseverance of fifteen or twenty minutes that the circulation became sufficiently restored to make it certain that life was safe. The operation was consequently postponed for a time. Upon this case Mr. Stanley remarks:]

It cannot be doubted that in this instance death was very nearly occasioned by chloroform. The phenomena were not either the stertor of oppressed breathing or the sudden stoppage of breathing from the flow of blood into the glottis: they were the loss of consciousness and motion followed by sinking of the circulation, apparently to its complete cessation. The phenomena observed in this and in other similar instances show the prejudicial agency of chloroform to be directly upon the heart. Accordingly, the circulation stops before breathing, and upon recovery the circulation returns before the act of breathing, circumstances that were well observed in the following case, the history of which Mr. Paget has been so good as to draw up for me:—"In a case of tetanus, which appeared to be idiopathic, and of which the symptoms were extremely severe, a man inhaled about two drachms of chloroform, and in an ordinary time and manner was brought under its full influence. I was trying to open his mouth, but his jaw was still fixed; and while still trying, we saw him become extremely pale, and then bluish about his lips and nose; then he began to breathe heavily and frothed at the mouth; and his pulse, which had been irregular during the inhalation of the chloroform, suddenly stopped: his heart ceased to beat, and in a few seconds more he ceased to breathe, and became motionless, with his limbs flaccid. He was to all appearance dead, and thus he remained for about a minute and a half, when again his heart began to act, blood returned to his face, and his pulse again became full; then he began to breathe again, and presently he breathed freely. In a few minutes more he had recovered from the effects of the chloroform, and relapsed into the tetanic state. While he was apparently dead, various means were used to resuscitate him, including sharp blows with the open hand upon his thighs and other parts, and these appeared to be useful. It was very observable that the ill effect of the chloroform was exercised on the heart more than, and earlier than, on the muscles of respiration. His heart ceased to act before he ceased to breathe, and his heart recovered and acted forcibly, so as to propel blood well into the capillaries of the face, before he began to breathe."

Each of the several measures employed in the case which I have related probably did something towards the recovery of the patient's life. But let us suppose that these measures had failed,—that by the free and persevering administration of brandy the pulse at the wrist had not become so distinct that life could be considered safe, the patient still continuing to all appearance lifeless. Under these circumstances, one other measure would, I think, have been tried, however doubtful its result, namely, artificial breathing, either through a tube passed from the mouth into the glottis, or through an opening made into the larynx. Some hope there would be of the success of artificial breathing in such a case from the fact that, in asphyxiated animals, it is found to be the most direct and sure means of exciting the action of the heart.

It will be a question whether, in this case, the ill effects of chloroform were owing to the excessive quantity of it used, or to the mode of administering it, or to an unusual susceptibility of its influence. My first impressions upon the occurrence of this case were, I confess, of so much alarm at the use of chloroform, that I felt inclined to return to the employ of ether as the anæsthetic agent. But upon narrowly viewing all the circumstances, I now adopt the conclusion, that in this instance the ill effects of the chloroform are to be attributed to the mode in which it was administered, and which I am disposed to explain in the following way. It is to be observed, that not until the ordinary inhaling apparatus was changed for the hollow cone of lint moistened by the chloroform did the alarming symptoms appear, and I cannot but think that from the lint thus imbued with chloroform the vapour was inhaled in too concentrated a form, not sufficiently mixed with fresh air to be

drawn into the air passages of an individual who had already inhaled it through the ordinary apparatus for twelve minutes.

Observation warrants the opinion that there is not any marked difference in the effects of chloroform upon different persons, although the time required to produce complete anæsthesia varies greatly, and the symptoms are not always the same which precede the suspension of consciousness and voluntary motion. Provided that due caution be used, chloroform may be administered with equal safety in infants and in advanced age; bnt in infants the action of the concentrated vapour of chloroform is so quick that great care is necessary in respect to the quantity of it used and to the mode of administering it, otherwise there will be danger of the action of the infant heart stopping simultaneously with the cessation of motion and sensation in the rest of the body.

Experience has not furnished evidence against the use of chloroform in instances of extreme debility, the consequence of long suffering from diseases of the joints or other organs. Although in such individuals the pulse may be extremely frequent and feeble at the time of the operation, yet it has not appeared that the chloroform is more likely to affect the action of the heart than when administered in opposite conditions of the system. Nor has it appeared that the use of chloroform should be forbidden when the nervous system is depressed by the shock of a severe injury, as a lacerated limb, compound fracture, or dislocation. Such, at least, is my impression from the observation of its effects in these cases.

An empty stomach is essential preparatory to the administration of chloroform. Our rule in the hospital is that the patient should take only a light breakfast several hours previous to the time of the operation. When, from inattention to this point, the chloroform has been administered upon a full stomach, the usual result has been, that shortly after the commencement of the inhalation, repeated ejections of the contents of the stomach have ensued, occasioning much exhaustion and distress to the patient. After the fullest and most protracted administration of chloroform, no other attention is ordinarily required than the abstinence from food for five or six hours. If food is taken within this period it will probably be rejected. Headache and continued irritability of stomach are the occasional ill consequences of chloroform, but they usually subside of themselves. If these symptoms continue with depression of the pulse, it may become necessary to administer brandy, ammonia, liquor opii, sedatives, &c. But rarely indeed are these measures required. Almost constantly on visiting the patient who has just undergone a severe and protracted operation, I have found the pulse of its natural fulness, force, and frequency. The general result of the use of chloroform in operations, is to leave the patient in a state neither of debility nor depression.

All the evidence to be gathered on this subject leaves my mind satisfied of the propriety of continuing the use of chloroform in operations.—*Medical Times, Nov.* 22, 1851, *p.* 529.

175.—POISONING WITH ARSENIC—RECOVERY FROM A LARGE DOSE—MAGNESIA AS AN ANTIDOTE.

By Dr. Douglas Maclagan.

[Dr. Maclagan was requested to see Margaret Davidson, aged 35, on the 4th of Nov., 1851, she having, at 3 o'clock, p. m., taken a dessert spoonful of powdered arsenic, in mistake for a saline effervescing powder. No effects were produced for at least half an hour, and then she experienced some sickness. At seven, p. m., she presented all the symptoms of irritant poisoning. Dr. Maclagan procured a Florence flask, some muriatic acid, a bit of copper wire, and a test-tube, and in less than ten minutes, by Reinsch's process, had procured an abundant sublimate of arsenious acid from the vomitings. Dr. Maclagan continues:]

Dr. Weir, previously to my arrival, had sent for a supply of "ferrugo," but, though several laboratories were applied to, none was to be found ready. Under these circumstances, it was determined, instead of preparing the hydrated sesquioxide by precipitating a quantity of the tincture of muriate of iron, to give a trial to the other reputed antidote of arsenic, magnesia. I therefore returned to the *patient, carrying* with me four ounces of light magnesia. I stirred it in as much

water as gave it the consistence of thick cream, and administered the magma at intervals of some minutes, in doses of two or three tablespoonfuls. It was generally vomited as soon as swallowed, the vomitings being quite white from magnesia. There was some difficulty in getting her to take it, as she thought that it was the cause of her vomiting; but by dint of some persuasion this was accomplished, and in the course of three-quarters of an hour, she took the whole four ounces, and I have little doubt vomited it all up again. No further treatment was pursued that night, than applying heat to the extremities, a large sinapism to the belly, and allowing, at her request, cold water in restricted quantities, not more than a wine glassful at a time, to relieve the intense thirst.

Nov. 5th. Has vomited all night, and still does so, but at longer intervals. She has since midnight had several loose stools, with slight heat at the anus. Has passed no urine since swallowing the poison, and it was ascertained by the catheter that the bladder was empty. She lies in a drowsy, torpid condition, eyes sunk, face blue, and, like the extremities, cold and clammy. She presented the most perfect resemblance to a case of Asiatic cholera in the stage of collapse. The pulse 120, very small; the tongue was red on the edges, with a peculiar white crust in the centre. She complained now of burning heat in the throat and mouth, and much tenderness of the belly. She was, at her own request, allowed butter-milk as drink, and she was ordered a solution of five grains of nitrate of potash in ℥i. of mucilage every two hours, and to have two teaspoonfuls of gin at the intervening hours.

Same evening she appeared much worse, the drowsiness was greater, the pulse feebler; the eyes injected. She had, at intervals, vomiting, with much flatulent eructation during the day, and thought this was caused by the nitre. There was no more diarrhœa, still no urine. A teaspoonful of spirit. etheris nitrici, with an equal quantity of spirit. ammoniæ aromaticus, was given in mucilage instead of the nitrate of potash, and a large blister was applied to the epigastrium. She appeared to me to be gradually passing into a state of coma, and I thought that she was dying.

6th. She was this morning in a state of re-action, the skin having become warm and moist; the pulse 96, soft; and she had passed four ounces of urine early in the morning, the suppression having thus lasted about thirty-six hours. She had some return of diarrhœa. In the evening the skin was hot and dry, the pulse 120, rather full, and she had been vomiting some green bile, mixed with dark patches, apparently of grumous blood. The sweet spirit of nitre, but without spirit. ammoniæ, and gin, in mucilage, were continued.

The urine passed this morning was examined chemically. It was deep brown, of density 1015, and strongly acid. It gave a large amount of albumen on heating, and a copious deposit of purpurine on the addition of hydrochloric acid to it when warmed. It was placed in a Marsh's apparatus, and gave a copious arsenical crust.

[On the 7th, she had no tenderness on pressing the abdomen, and the pulse was 120. On the 8th, she was allowed a little beef tea, and, as she was restless at night, a little morphia was given. On the 9th is stated to have passed 36 ounces of urine: pulse 90, of good strength: still a little vomiting. On the 10th, no vomiting. On the 11th, during the day, she was seized with vomiting, and brought up about 10 ounces of a dark grumous bloody fluid, but no tenderness of the abdomen; the pulse rose to 120 before night. On the 12th, the pulse still 120, and extensive bronchitis over the whole of both lungs. Is to take 8 minims of ipecacuan wine, with 5 ôf solution of morphia, every two hours. Twelve ounces and a half of urine, subjected to Marsh's process, gave a considerable arsenical crust. On the 19th, 24 ounces gave a small but very distinct crust of arsenic. On the 25th, 26 ounces gave a faint arsenical crust.]

The diagnosis here was, practically, not a matter of any difficulty. It was founded essentially upon the agreement of the symptoms with the girl's own statement that she had swallowed arsenic. It was, however, confirmed and rendered indisputable by the application of Reinsch's process to the vomitings; and, without attaching too much importance to the particular treatment followed here, yet I may observe, that it was adopted and persevered in mainly upon the positive assurance, which the analysis of the vomitings gave, as to the real nature of the case, and that whether the recovery is to be called its effect or no, it was, at all events, the

sequence. I am not aware of having observed that Reinsch's process has ever been applied as a means of diagnosis, but assuredly in doubtful cases it is susceptible of such application; and it would certainly be satisfactory to a medical practitioner, called to a case which should afterwards prove fatal, and be determined by post-mortem analysis to be one of poisoning, that he had positively assured himself of this during the life of the patient, and had treated it as such. In relation to cases of recovery, it might also be not without its use, as enabling us to judge of the value of any particular treatment. Many of the observations adduced as testimony to the powers of an antidote are rendered worthless, by its not having been positively ascertained that the poison really had been swallowed. An instance of this exists in the last Number of this Journal, respecting a recovery, by the hydrated oxide of iron, of a supposed, but most improbable, case of poisoning by arsenic. The application of Reinsch's process to a portion of the vomitings, during the progress of the case, offers no practical difficulty,—it will, in general, be easy to procure the materials, and, of course, absolute purity in the hydrochloric acid is not required here, as in a strictly medico-legal analysis. In no town, scarcely in any village, would it be difficult to find some "spirit of salt," and a bit of copper; the glass tube for sublimation is, in reference to diagnosis, unnecessary. If the piece of copper be crusted black during the boiling, and when heated in a candle lose its crust, and give off alliaceous fumes, the evidence will be sufficiently precise.

The result of this case entitles us to place it among the facts adducible in support of the employment of magnesia as a remedy in arsenical poisoning. I do not say as an antidote in the chemical acceptation of the word, because the doses of magnesia remained so short a while in the stomach, that it could hardly have time to form an insoluble compound with the arsenic, admitting that it is capable of so doing, of which the experiments of Bussy and Christison leave no doubt. My own impression at the time was, that if the magnesia had any special effect at all relative to the poison, it was chiefly mechanically, by enveloping the particles of arsenic, and, emetically, by exciting the vomiting, and thus procuring the speedy ejection of the poison from the stomach. I am not disposed, in reference either to this or any other case of arsenical poisoning, to maintain that the magnesia (and the same remarks apply to the hydrated oxide of iron), if its use be followed by success, acts purely as a *chemical* antidote, because, as has been shown by Dr. Taylor, in his able discussion on this subject, in his book on "Poisons," these agents do not affect the solid arsenic in the stomach, but only precipitate, and that to a small extent, the arsenic that is in solution. At the same time, it is a fact not to be entirely lost sight of, that both ferrugo and magnesia do, when given in large quantity, form insoluble compounds with arsenious acid. My own experiments 'Edin. Med. and Surg. Journal, liv.,' point to twelve parts of the oxide; and Dr. Cristison's 'Monthly Journal, August 1846," to twenty-five and upwards, of magnesia to one of arsenic, as the proportions requisite to effect this precipitation. Whilst, therefore, I do not look upon ferrugo and magnesia as antidotes to arsenic in the same chemical sense in which chalk is to oxalic acid, and whilst I believe that a great part of their supposed beneficial effects is to be ascribed to their mechanical action upon the undissolved arsenic, I am not inclined to throw their chemical behaviour to the poison entirely overboard; and I therefore cannot entirely subscribe to my friend Dr. Taylor's remark, that whilst, "as a bulky mass it may serve mechanically to suspend the poison, and thus facilitate its ejection from the stomach, it possesses no advantages over albumen or other viscid liquids." 'Poisons, p. 334.' It must be remembered, that the arsenic which enters the circulation, and which is the real source of the danger, is in ponderable quantity by no means great. That, therefore, a very small quantity rendered insoluble becomes of importance to the safety of the patient. Further, the "danger in the absorption of poisons appears to arise less from the absolute quantity taken up by the blood, than the quantity admitted into the circulation at any one time" [Taylor]; and that therefore any means which can, even to a trifling extent, diminish absorption at the time that it is going on most actively,—*i. e.*, when the stomach is full of poison, may be of consequence. The *chemical* value of the antidote ought, I think, to be viewed, not, as is generally done, in relation to the dose swallowed, but in *relation to the quantity* undergoing absorption at the moment; and if two or three

grains of arsenic absorbed be enough to peril life, two or three fractions of grains kept out of the blood may be of no small consequence in relation to the chances of recovery. I attach no importance to the objection, that those compounds formed with arsenic by the oxide of iron and the magnesia, are soluble in the acid juices of the stomach. There can be no free acid capable of effecting such a solution in the secretions of a stomach which is filled with basic matter, such as oxide of iron or magnesia. It is perhaps by its neutralizing the gastric acids, that the moist oxide of iron prepared by ammonia, a little of which it always retains, is superior to the oxide prepared in other ways, or dry.

The above considerations, in connection with this additional case, where the use of magnesia was followed by recovery, and the fact, that neither it nor the oxide of iron do any harm, ought therefore, I think, to encourage us to use these reputed antidotes freely, provided always that where vomiting is not going on, these are not relied upon to the exclusion of the infinitely more important object of getting the poison out of the stomach.—*Monthly Journal of Med. Science, Feb.* 1852, *p.* 124.

176.—*Internal Inflammation Combated by Collodion.*—M. Latour has been for some time back the persevering advocate of the doctrine, that external inflammation may be speedily subdued by withdrawing the affected surface from the influence of the air. This he obtains simply by a layer of collodion, which (according to M. Latour) rapidly assuages the inflammation of gout and articular rheumatism. At the meeting of the Academy of Medicine, on the 8th, M. Latour related a case of peritonitis, in which the symptoms were dissipated within twenty-four hours by the application of a layer of collodion to the whole surface of the patient's abdomen.—*Bulletin de l' Académie.—Med. Times and Gazette, March* 13, 1852, *p.* 270.

177.—ON THE PROPERTIES AND USES OF GUARANA.

Communicated by Dr. Ritchie, R.N.

[This substance was presented to Mr. Ritchie by a Brazilian, and from a statement of its virtues he was induced to employ it in several troublesome and obstinate diseases. The following is a description of the drug.]

The term Guaraná is derived from the name of a tribe of Indians, who are dispersed between the rivers Parana and Uruguay, by whom it is very commonly used as a condiment or medicine. It is, however, more extensively prepared for commercial purposes by the Mauhés, an Indian tribe in the province of Para and of Tapajoz, on the banks of the rivers Mauhé, Maragnon, and Tapajoz. It is, according to Martius, prepared from the seeds of the Paullinia sorbilis, a species belonging to the natural family Sapindaceæ. The characters of the species are:—Glabra, caule erecto angulato, foliis pinnatis bijugis, foliolis oblongis, remote sinuato-nudo angulato, racemis pubescentibus erectis, capsulis pyriformibus apteris rostratis, valvulis intus villosis. The seeds, which ripen in the months of October and November, are collected, taken out of their capsules, and exposed to the sun, so as to dry the arillus in which they are enveloped, that it may be more readily rubbed off by the fingers. They are now thrown upon a stone, or into a stone mortar, and reduced to powder, to which a little water is added, or which is exposed to the night dew, and then formed by kneading into a dough. In this condition it is mixed with a few of the seeds entire or contused, and divided into masses, weighing each about a pound, which are rolled into cylindrical or spherical forms. These are dried by the sun or by the fire, and become so hard as to be broken with difficulty. Their surface is uneven, brown, or sometimes black, from the smoke to which they have been subjected; their fractured surface is conchoidal, unequal, and resinoid: colour reddish-brown, resembling chocolate. This is the guaraná, and in this condition, or reduced to powder, it is kept for use or carried to market. The museum of the Edinburgh College of Physicians contains a specimen of it in each of these forms. As it is liable to be adulterated with cocoa or mandiocca flour, it is of importance to be aware that the genuine article is the

tinguished by its greater hardness and density, and in that, when powdered, it does not assume a white colour, but a grayish-red tint.

A chemical analysis of this substance was first made by Theodore Martius in 1826 (Buchner's Repert. de Pharm., xxxi., 1829, p. 370.) He found it to consist of a matter (tannin?) which iron precipitated green, resin, a fat green oil, gum, starch, vegetable fibre, and a white bitter crystalline product, to which the efficacy of the medicine was principally owing, and which he named guaranine. This he believed to be distinct from, but allied to, theine and caffeine, and to possess the following elementary constituents,—$C_8 H_{10} O_2 N_4$.

Another very careful analysis of guaraná was made in the year 1840, by MM. Berthemot and Dechastélus (Journal de Pharmacie, tom. xxvi., p. 518, *et seq.*), which varies in some degree from the preceding. They found the matter, which was considered to be resin by Martius, a combination of tannin with guaranine, existing in a form insoluble in water or ether. They also determined the perfect identity of the crystalline matter with caffeine. It is found to exist in a much larger proportion in the fruits of the Paullinia than in any of the plants from which it has hitherto been extracted. Alcohol is the only agent which completely removes it from the guaraná. To this solution the addition of lime or hydrated oxide of lead gives, on the one hand, the insoluble tannates, and on the other, the crystalline matter.

The medicinal virtues of this substance have been attentively examined by Theodore Martius (Op. cit.), and more particularly by Dr. Gavrelle (sur une nouvelle substance médicinale, etc.: Paris, 1840), who employed it very often while in Brazil, as physician to Don Pedro, and afterwards in France. By both it is considered a very valuable remedy, and an important addition to the materia medica. By the vulgar it is held to be stomachic, antifebrile, and aphrodisiac; is used in dysentery, diarrhœa, retention of urine, and various other affections. It stimulates, and at the same time soothes, the gastric system of nerves. It reduces the excited sensibility of the cœliac plexus, thereby diminishing febrile action, and strengthening the stomach and intestines, particularly restraining excessive mucous discharges, increasing the action of the heart and arteries, and promoting diaphoresis. It is therefore indicated as a valuable remedy in fevers, or reduced vital power resulting from cold or prolonged wetness, grief, too great muscular exertion, depression of spirits, long watching, and also in colic, flatulence, anorexia, nervous hemicrania, or in a dry condition of the skin. It is contra-indicated in a plethoric or loaded condition of the abdominal viscera, and when there exists determination of blood to the head. It is said to increase the venereal appetite, but to diminish the fecundating power.

In cases where irritation of the urethra or urinary bladder succeed venereal or attend organic disease, it exerts a most salutary effect in soothing the irritability of the mucous membrane, relieving the nervous prostration which accompanies these affections, and exalting vital power. Unlike the disagreeable remedies which are generally, and often without success, employed in these affections, it is taken with pleasure, and with an amount of success which, as far as my experience extends, is universal.

If we examine guaraná according to its chemical characters, it must be regarded as a most valuable substance, from its possessing in so great a proportion that important nitrogenous principle guaranine. This, if not identical with caffeine, is at least analogous to it, and to theine, and theobromine,—all important elements of food and grateful stimulants. From its chemical constitution, then, we may predict with great certainty its physiological action as being powerfully tonic; but, in the combination in which it is found, experience indicates that it possesses conjoined more valuable properties than belong to the simple tonics. Its power of correcting generally the discharges, and restoring the normal vitality of the mucous membranes, must be viewed as one of these.

Guaraná, in the state of powder, is exhibited in doses of 3j. three or four times daily, mixed with water and sugar, or with syrup and mucilage, conjoined with an aromatic, as cinnamon, vanilla, or chocolate. A convenient form is that of extract, obtained by treating the guaraná with alcohol, and evaporating to the *consistence of pills*. This may be exhibited in the form of solution or pills. *The Brazilians*, however, use the powder with sugar and water alone, and consider

this draught grateful and refreshing.—*Monthly Journ. of Med. Science, May* 1852, *p.* 465.

178.—ON THE CAUSE AND PREVENTION OF DEATH FROM CHLOROFORM.

By Dr. Snow.

[At a meeting of the Medical Society, of London, Dr. Snow read a paper on this subject:]

He said, that, by inhaling too much of this agent, the sensibility might be so much diminished that the necessity for breathing was no longer felt, and the action of the heart became afterwards arrested for want of respiration. Under certain circumstances, also, chloroform stopped the motion of the heart by its direct influence. He had observed the manner of dying, in numerous experiments on dogs, cats, and other animals, with the stethoscope applied to the chest, while the quantity of vapour of chloroform in the air they were breathing was known. When they were killed by breathing air containing from 3 to 5 per cent. vapour, the inhalation lasted ten or fifteen minutes, and the breathing ceased about a minute before the action of the heart. In some instances, the animals made two or three gasping inspirations at the time when the action of the heart became inaudible: and, if the chloroform were withdrawn, these gasps had often the effect of restoring the animals. When the vapour was present to the extent of 8 per cent. or upwards in the air, the death of the animals was very sudden, and the breathing and action of the heart ceased together, except in a few instances, in which the respiration actually continued after the heart had ceased to beat. In all the accidents which had happened to the human subject, death had taken place by sudden syncope; and it was evident that the action of the heart had been paralysed by the effect of the vapour of chloroform present to the extent of not less than 8 or 10 per cent. in the air breathed by the patient within a minute before the accident. Dr. Snow enumerated the deaths which had occurred from the administration of chloroform: they amounted to eighteen. In sixteen of these cases, the agent had been exhibited on a handkerchief, or towel, or piece of lint; and in the remaining two, in which some form of inhaler had been employed, it was not used by a medical man. The subjects of these cases were in a better state of general health than the average of persons who require to inhale chloroform. None of them were children or old people. Only one was reduced to a state of debility; and the operations that were being performed, or were about to commence, were, with two or three exceptions, of a trifling nature. Chloroform was so powerful, that it required special means to ensure that its vapour should be well diluted with air; not for the purposes of respiration, but to prevent its acting with such rapidity that there should be no time to watch its effects, and to prevent its ever being present in the lungs in such quantity as to paralyse the heart, by overcharging a portion of the blood. There were two ways of ensuring the proper mixture of air with the vapour: the first and best was to employ undiluted chloroform with a suitable inhaler; the second was to dilute the chloroform with an equal quantity of rectified spirit of wine before pouring it on a handkerchief or sponge,—for by this means the quantity of vapour which it would yield to the air, would be diminished to within safe bounds. In cases of accidents from chloroform, he considered that the best means of preventing death was artificial respiration. From experiments which he had made on animals, he considered that, if it were performed within half a minute after the apparent death of the patient, it would in most cases be successful. He thought that the most safe and prompt method of performing it, in the human subject, would be to apply the mouth to the nostrils of the patient, and draw as much air as possible from the lungs, allowing them to be filled again by the elasticity of the ribs and atmospheric pressure. If this measure should not quickly succeed, he would recommend that the external jugular vein should be opened while the artificial respiration was still continued: for he had observed, in his experiments, that the right cavities of the heart became distended when its action failed, and that opening a vein near the heart improved the force and extent of any contraction which still continued. He

had tried electricity on animals without much success.—*Med. Times and Gazette, Mar.* 6, 1852, *p.* 253.

[Mr. BULLOCK remarked, that the bad effects of chloroform in many instances he attributed to the impurities with which it was contaminated. It is made from wood-spirit as well as alcohol, and when so made, a volatile oil is generated, which cannot be separated, and which is very injurious. Again, sulphuric acid is sometimes present, which liberates chlorine, which is also inimical to health. In regard to the impurities of chloroform, Dr. MURPHY said he had a very simple test:—Chloroform, when rubbed upon the palm of the hand, left a fragrant odour, in no way pungent; any pungent odour led him to suspect its purity.]

179.—*Electricity recommended for Arresting the Fatal Effects of Ether and Chloroform.*—DR. ABEILLE, of Corsica, advises, in a paper presented to the Academy of Sciences, electricity as a means of reviving patients on the point of dying from the effects of chloroform. The author states that anæsthesia is produced by a momentary disturbance of the cerebro-spinal system, and he advocates the use of electricity, to be applied by means of needles thrust into various parts of the body, especially over the cerebro-spinal axis. The insensibility immediately disappears, and the muscles in contact with the needles begin to act. Dr. Abeille considers that this is the only means to be depended upon in occurrences of the above-mentioned kind.—*Lancet, Nov.* 29, 1851, *p.* 509.

180.—*On the Topical Use of Chloroform.* By DR. RAUCH.—To obviate the volatile character of chloroform when employed topically, Dr. Rauch combines it with olive oil and some liquor ammoniæ, forming an emulsive liniment. This is less expensive, relieves sooner, and is not so volatile as chloroform. The ingredients were at first employed in equal parts; but were afterwards used in other proportions, according as to whether a counter-irritant effect (when more ammonia and chloroform must be added) were desired or not. It is applied on a woollen cloth, so folded that the inner layer is saturated by the liniment, and the outer kept dry, so as to prevent evaporation. When first applied, it feels cool, then smarts and burns for ten minutes so as hardly to be borne; and then an agreeable coolness, with relief of pain, succeeds. When it causes too much irritation or vesication, it should be removed, or applied to another locality. The skin is made red by it, and often vesicated; and if a mere rubefacient is required, it should be applied by friction, or the cloth should remain on only for a short time. When a speedy vesicant effect is required, it is more useful than a sinapism or blister, and is easier of application, especially in children, who often fall asleep during its application. Dr. Rauch found it of great use, combined with other means, in cholera; and in relieving the painful affections of the abdomen in children, it is preferable to any anodyne. In the case of superficial burns, a compound of equal parts of chloroform, olive oil, and lime water, has been found highly useful.—*Amer. Jour.*—*Brit. and For. Med. Chir. Review, Jan.* 1852, *p.* 274.

181.—ON NARCOTISM BY THE INHALATION OF VAPOURS.

By DR. JOHN SNOW.

For a length of time after the changes which are effected in the air by respiration were discovered, it was generally believed that the carbonic acid was formed in the lungs, by the union of the oxygen of the air with carbon contained in the blood; and the phenomena of asphyxia were thought to be occasioned by the direct action of some form or combination of carbon which ought to have been excreted. Experiments by Edwards, and others, on the respiration of animals in hydrogen gas, and especially the beautiful experiments of Professor Magnus on the blood, clearly proved, however, what many physiologists had believed from the first,—that the oxygen of the air is absorbed (along with some nitrogen) and circulates with the *arterial blood,* combining with carbon in the systemic capillary circulation, and thus *forming the carbonic acid* which is exhaled from the blood in its passage through

the lungs. Asphyxia is simply due to the want of oxygen in the arterial blood; for, although there is a little carbonic acid gas present in this blood during the more ordinary forms of asphyxia, yet the same symptoms occur to animals placed in hydrogen or nitrogen gas, although the carbonic acid gas in the blood is then exhaled. The presence of oxygen in the blood seems absolutely necessary to the performance of the animal functions—so necessary that none of them can continue an instant without it. Animals live, it is true, for a short time after they are deprived of air, but a little consideration shows that they live only by virtue of the oxygen which is contained in their bodies, and that when this is consumed life no longer continues. The length of time which animals live after they are deprived of air is in the inverse ratio of the activity of their functions, and Dr. W. F. Edwards has shown that animals of cold blood, as reptiles and fishes, die of asphyxia, nearly as quickly as animals of warm blood, when they are placed in water deprived of air, and of a temperature of about 100° Fah. The increase of heat quickens the changes taking place in the body, as the same author has proved by distinct experiments; the oxygen dissolved in the fluids of the animal is soon appropriated, and life is then extinct. Animals of cold blood can also be quickly killed at the ordinary temperature by the rapid absorption of agents, such as the vapour of ether, which have the undoubted power of arresting oxidation out of the body, and when present in the blood in sufficient quantity, have the effect of preventing the oxygen it contains from any longer entering into combination. The experiments of Dr. Kay show that venous blood has some power of supporting the functions of the brain, and the irritability of the muscles when injected into arteries, but this depends on some free oxygen it contains; for the analyses of Magnus have proved that arterial blood is only deprived of part of its oxygen by passing once through the systemic capillaries.

The relation between asphyxia and narcotism is this—that in asphyxia there is an absence of oxygen, whilst in narcotism the oxygen is present, but is prevented from acting by the influence of the narcotic. With this close affinity between asphyxia and narcotism, as regards their intimate nature, there is, as might be expected, a great similarity in the phenomena of the two conditions. The different parts of the nervous centres lose their power, under the influence of ether and chloroform, in the same order as in asphyxia. The action of the heart continues in asphyxia after the muscles of respiration have ceased to contract, and this is the case under the effects of chloroform, alcohol, ether, and probably all narcotics, when they are absorbed in a gradual and uniform manner. For, as the muscular contractions of a peristaltic character, which are under the influence of the ganglionic system of nerves, can go on with a smaller amount of oxygen than those which are dependent on the cerebro-spinal system, so it requires a larger quantity of the narcotic to arrest them.

During sudden asphyxia of robust subjects by privation of air, there are generally convulsions after the loss of consciousness, and there is likewise usually an amount of muscular rigidity and contraction approaching to convulsions when insensibility is quickly induced by chloroform or ether, in muscular persons, or robust animals. By gradually inducing narcotism these contractions can be avoided, and in like manner, when asphyxia is slowly induced by vitiation of a limited supply of air, convulsions are not induced. The impediment offered to the absorption of oxygen in the lungs during bronchitis is sometimes accompanied by delirium not unlike that caused by a narcotic, and occasionally coma is met with. The state of the fœtus in utero—just able to perform a few languid movements of its limbs—resembles very much the sleep caused by a narcotic. At this time it receives only a limited supply of oxygen at second hand through the placenta; but on being born, no sooner has it taken one or two free inspirations, than it exhibits an amount of activity and strength which would be fatal to the mother did it possess it whilst in the womb.

With all these points of resemblance between narcotism and asphyxia, it might perhaps be asked why a limitation of the supply of air, or in other words a partial asphyxia, might not be resorted to instead of a narcotic to prevent the pain of operations. The answer must probably be sought in the circumstance remarked by all the observers of the phenomena of asphyxia, that the blood becomes arrested at the pulmonary capillaries, when oxygen is no longer admitted into the

air-cells of the lungs. On this account insensibility cannot be induced by means of asphyxia, without causing congestion of the lungs, and great distress of the respiration.

In a profound state of narcotism the symptoms often exactly resemble those of apoplexy. In both conditions there is a partial suspension of the process of oxidation on which the functions of the brain depend; but this impediment to the natural process of oxidation arises from a different cause in the two cases. In narcotism it is due to the presence of the narcotic substance in the blood, which retards oxidation, as we shall presently see, by a kind of counter affinity for the oxygen; in apoplexy it depends on more or less complete interruption to the circulation of the blood. For the constant action between the oxygen of the arterial blood and the brain, there is obviously required a never-ceasing current of blood, and when this is interrupted in any part of the brain, it is clear that there must be interference with the process of oxidation; and it matters not whether the circulation be interfered with by pressure arising from effusion, by the occlusion of one or more of the arteries which cuts off part of the supply, or by such an amount of congestion from any cause that the current of the circulation is interrupted. According to these views it ought not to signify whether there is increased or diminished pressure in the cranium, or whether the quantity of blood in the brain is more or less than natural; but if the circulation is interrupted or greatly impeded, there ought to be the symptoms which arise from impeded oxidation. Such indeed is the fact; we meet with the same symptoms in very different physical conditions of the contents of the cranium, and the question of bleeding and the application of other remedies cannot be decided by the cerebral symptoms alone, without the consideration of other particulars.

The circulation through the capillaries of the brain is undoubtedly sometimes retarded under the influence of narcotics; but this is the consequence and not the cause of the impeded functions of the brain. For, as was first pointed out by Professor Alison, the functions of the various organs of the body are accompanied by a force which aids the capillary circulation; and on the function of any organ being interrupted, the circulation through it is retarded, as is seen in the most striking manner in the lungs during asphyxia. There is this further difference also between narcotism and apoplexy, that the narcotic acts directly on all parts of the body as well as on the brain, whilst in apoplexy the remainder of the nervous system and the other organs of the body are only affected in a secondary manner.

In my last communication, several experiments were detailed which shew that the quantity of carbonic acid evolved from the lungs is considerably diminished under the influence of ether and chloroform. This circumstance indicates diminished oxidation, for carbonic acid is the chief product of that process in the animal frame, and it bears a pretty close relation to the amount of oxygen consumed. Dr. Prout formerly showed that the quantity of carbonic acid produced in respiration was diminished after drinking alcoholic liquors, and alcohol very much resembles ether and chloroform in chemical constitution and physiological effects. Under the influence of this agent, alcohol, Böcker ascertained, as was noticed before, that the amount of every one of the constituents of the urine is diminished, and phosphoric acid and urea are important products of oxidation.

In some experiments detailed in the first part of these papers, the temperature of animals was seen to diminish under the continued influence of ether and chloroform. This circumstance is also illustrative of the diminished oxidation that is taking place, for the experiments of Dr. W. F. Edwards on animals of various species, at different periods in their life, and in different seasons of the year, show that the consumption of oxygen in respiration always bears a direct proportion to the evolution of animal heat.

Gradual exposure to a lower temperature, as happens in the change of season from summer to winter, alters the constitution of many animals, causing them to consume more oxygen, and thus to develop more heat, and bear up against a colder season; but other species, including some mammalia, as well as nearly all reptiles, are narcotised by the cold, and fall into a state of torpor in the winter when the consumption of oxygen is reduced to a minimum. Cold air, or whatever *abstracts* the heat of the body, so as to make a considerable reduction in its *temperature, is* a true narcotic, and acts like other narcotics, by diminishing oxi-

dation. Travellers in the arctic regions inform us that the symptoms produced by intense cold are sometimes not to be distinguished from intoxication by alcohol, except by the circumstance that no spirituous liquors can have been obtained. As regards its local effects, cold is probably the narcotic which has been longest known to the human species; for its benumbing effects make themselves felt, in the fingers at least, in most parts of the earth, at some season of the year. The local application of cold closely resembles that of chloroform and many other narcotics, in causing a slight amount of pain before sensibility is altogether abolished. Dr. James Arnott, who has given great attention to the local effects of graduated temperature in the treatment of various affections, has relieved neuralgic pains by the application of a mixture of salt and pounded ice, and has also rendered the surface of the body so insensible, that the introduction of setons, and other operations of a superficial nature, have been performed without pain. Dr. Arnott calls the process congelation; but the hardness which is produced in the part must depend on the solidification of the adipose substance; for if the water which enters into the composition of the tissues were frozen, their intimate structure would be destroyed, and a slough would be the result.

The effects of ether and chloroform on the appearance of the blood agree perfectly with the view above given of their modus operandi. There is generally no alteration in the complexion of the patient, or in the colour of the mixed venous and arterial blood as it flows from a wound, so long as the inhalation is not pushed to the extent of embarrassing the respiration, and provided the patient is not holding his breath, on account of the pungency of the vapour, or a general state of rigidity which sometimes occurs for a minute or two; but when the blood which flows from the arteries and veins can be separately observed, whilst the patient is well under the influence of the narcotic, it is seen that the arterial blood is somewhat less florid, and the venous blood less dark than under ordinary circumstances. The lighter colour of the venous blood, which has been spoken of by Dr. Gull, as well as by myself, points particularly to a diminution of oxidation in the systemic capillaries.

The phenomena attending the irritability which remains in the muscles for a longer or shorter time after death, and particularly the effect of narcotics on this irritability, accord exactly with the views above expressed. It can be shown, by the following amongst other reasons, that the muscular irritability depends on a little oxygen still remaining in the blood contained in the muscular tissue. Nysten found that the injection of oxygen gas into the cavities of the heart increased the vigour and duration of the contractions. Sir B. Brodie states that, in dogs in which the circulation was kept up after death by artificial respiration, "there seemed to be actually an increased irritability of the voluntary muscles, continued not for a short time, but even for an hour and a half." Nysten informs us that the general result of his observations on the duration of the muscular irritability in animals of different classes, and of different orders of the same class, was in the inverse ratio of the muscular energy developed during life: and we previously saw, on the authority of Edwards, that this was just the ratio of duration of life under privation of air or asphyxia.

Chloroform, ether, alcohol, and probably all narcotics, have the power of suspending the muscular iritability. In a former paper of this series some experiments were related in which the irritability of the heart in frogs and rabbits was removed by the vapour of chloroform; and in two of the experiments the irritability was alternately allowed to recover by letting the chloroform evaporate and then suspended again by a fresh exposure to the vapour. In one of these experiments the peristaltic action of the small intestine of a rabbit was arrested by the local action of chloroform. I have frequently stopped the quivering motion of the intercostal muscles, which is seen on opening the chest of an animal immediately after death, by blowing a little vapour of chloroform on them through a tube. On one of these occasions Dr. Sibson was present.—*Med. Gazette, Dec.* 19, 1851, *p.* 1053.

182.—ON CERTAIN OXIDIZING PROCESSES GOING ON IN THE SYSTEM.

By Dr. H. Bence Jones, F.R.S., &c.

[If the reader will attentively study the following paper by Dr. Jones, he will perceive the value of oxidation in a degree in which he perhaps never did before. He will find that the whole life of the individual seems to consist more or less in this process. It seems to be busy in every part of the animal frame. Dr. Jones, in this paper, begins by giving us M. Baral's table on the formation of water in the system by the combined action of oxygen and hydrogen. This table is as follows :—]

Water going in daily.

Existing in Food.	Disposed to form in Food from Hydrogen and Oxygen.	Formed by Combustion of Oxygen of the Air with Hydrogen of Food.	Total.
Lbs.	Lbs.	Lbs.	Lbs.
4·409 ...	0·615 ...	0·413 ...	5·437
4·064 ...	0·443 ...	0·289 ...	4·796

Water going out daily.

In Urine.	In Excrements.	Total.	Hence perspired Water.
Lbs.	Lbs.	Lbs.	Lbs.
2·364 ...	0·234 ...	2·598 ...	2·839
2·158 ...	0·121 ...	2·279 ...	2·517

The total quantity of water which, ready formed or about to form, goes into the body, is 5·437 lbs.; and of this quantity, it may be said, on an average, that about one pound is produced in the body by the combination of the hydrogen of the food with the oxygen of the food and of inspiration. The oxygen goes in through the lungs and through the stomach, while the hydrogen enters through the stomach alone. Not only does oxygen combine with hydrogen and carbon, but it combines also with sulphur and phosphorus, and with the inorganic salts, if they be not already in the highest states of oxidation when they go into the body. The sulphur, the phosphorus, and the salts, are as much constituents of the body as the hydrogen or the carbon of which I have been speaking in my previous lectures. If oxygen is acting in the body, not only will water and carbonic acid be formed, but we shall have the products of the oxidation of the sulphur, the phosphorus, and the salts, if they can be oxidised; and it is my object in this lecture, especially to point out the evidence of these oxidations.

I have shown you that certain quantities of carbon and nitrogen are being removed from the body daily; and the same thing is perfectly true regarding the sulphur, the phosphorus, and the salts. Certain quantities are passing off daily, and the food contains a new supply of these substances, to make up for the quantity removed, just as we saw that it contained a supply of carbon and hydrogen to replace that consumed by the wasting and wearing of the tissues. Sulphur and phosphorus go into the body, as I have told you, combined with carbon, hydrogen, oxygen, and nitrogen, in albumen for example. You will remember, that in albumen, fibrin, and casein, sulphur is always present, forming nearly 2 per cent. of their constituents. The sulphur does not go in as sulphuric acid, but as sulphur,—as an unoxidised substance,—in a state, therefore, which admits of oxidation, which oxidation actually takes place in the course of its use and passage through the human body. So also, if sulphur be taken as a medicine, an increase in the sulphates in the urine will be found to occur. If an excessive supply of nitrogenous food, or of substances containing sulphur, phosphorus, or salts, be taken into the body, a portion of the excess will be thrown out directly by the urine without entering into the composition of the tissues. The total quantity of the sulphur and phosphorus met with in the urine does not, therefore, *necessarily* come from the tissues themselves. The total quantity found in *the urine might* come from the food, and not directly from the tissues. Thus it is

necessary, if you wish to know how much sulphur and phosphorus actually passes out of the body from the tissues, to determine first the influence of the food, and then to determine the quantity that comes from the tissues, by deducting the amount of influence of the food, as far as possible, from the total quantity passing out of the body. We can easily make experiments to determine what the influence of the food actually is—that is, whether, soon after a meal, the quantity of sulphur and phosphorus in the urine is increased or decreased. If it be distinctly increased after each meal, it is very clear, or at least it is in the highest degree probable, that the increase must come from the food, and not from the tissues. Thus we must separate between the two sources. Regarding the quantity of sulphur and phosphorus passing off from the body, it is necessary to determine the quantity that occurs in health, the quantity that is produced by taking food, and the quantity formed when no food is taken. I can collect the quantity of urine passing off in 24 hours, and can throw down the sulphuric acid present by chloride of barium, and determine its whole amount. This is by no means difficult in healthy persons; but it is difficult, in the case of disease, to collect all the urine secreted in exactly 24 hours. It is in cases of disease, such as those that occur in hospitals, that these experiments are most frequently made, and there only can they be made in sufficient number. In hospitals it is difficult to follow this investigation. In fact, the difficulty of collecting the whole quantity secreted in exactly 24 hours is so great, that it may be said to be almost impossible to be accurate. Thus I have been obliged to take a certain quantity of urine at a fixed hour, as near as possible, to determine its specific gravity, by which some comparison may be made as to the probable quantity of urine that is passed; for the higher the specific gravity, the less water probably will be secreted in 24 hours; and the lower the specific gravity, the more water will be made. By this means the results are not dependent on the accuracy of the patient who is the subject of the experiments. It was not until I found the practical difficulties of collecting the urine in diseases, that I determined, instead of taking the whole quantity in 24 hours, to take a certain quantity at a known period, and then to record the amount of sulphur and phosphorus which was present at that period.

In health, the urine passed soon after food, has the specific gravity of about 1033·9 to 1029·3. This is the case of a healthy man in full diet, and with full exercise. Such urine will, with chloride of barium, give a certain quantity of sulphate of baryta. If I take a 1000 gr. bottle, and fill it with urine, determining its specific gravity, and if I treat it with chloride of barium, sulphate of baryta will be precipitated. And if I boil it with hydrochloric acid, to insure the solution of the phosphate of baryta, and then throw the precipitate on a filter and burn it, I can accurately determine the quantity and the weight of the sulphuric acid which existed in it. By this means I find, that in 1000 grs. of urine I generally get from 15·23 grs. of sulphate of baryta to 9·49 grs. per 1000 grs. of urine. That is my standard for health soon after food is taken. In the urine which is passed long after food, just previously to a meal being taken, when no food has for some time been in the stomach, and after free exercise has been taken, I find the specific gravity is from 1027·6 to 1025·3; and there is then not so much sulphate of baryta precipitated. It varied long after food from 8·56 grs. per 1000 to 7·07 per 1000 grs. urine; showing that the food distinctly produces an effect in increasing the amount of sulphuric acid in the urine; that it distinctly increases the quantity of sulphate of baryta which can be precipitated from the urine. Similar experiments can be made with regard to the phosphates. If I precipitate the urine simply by the addition of ammonia, I shall throw down only the earthy phosphates, that is, the phosphates of lime and magnesia, on which I shall dwell much more in detail in a future lecture on this subject. But if I add a little lime-salt to this urine, I decompose the alkaline phosphates, and, by converting them into earthy phosphates, cause them also to fall as a precipitate when pure ammonia is added. I have here two portions of urine, both precipitated by ammonia; but to one of them a little muriate of lime has been added, and you see a very decided difference in the amount of the precipitates; one shows the earthy phosphates alone, and the other the earthy and the alkaline phosphates which have been converted into earthy phosphates by adding chloride of calcium. In the one case, I have only phosphate of lime and phosphate of magnesia; in the other, I have decomposed the phosphate of soda and precipitated the phosphoric acid, by means of its combination

with lime. By throwing the two precipitates on filters, and burning them, I can arrive at the quantity of earthy phosphates, and also at the total quantity of earthy and alkaline phosphates in the urine.

By such experiments on the total quantity of phosphates precipitable from 1000 grs. of urine in health, I find that, soon after food, the phosphates vary from 6·42 grs. to 7·96 grs. per 1000 grs. urine, the sp. gr. being 1027·3 to 1025·5. Long after food, when the sp. gr. of the urine was 1022·8 to 1028·0, the phosphates are from 6·82 grs. per 1000 grs. to 8·85 grs. per 1000 grs. urine; indeed, it is doubtful whether they are increased at all, as you will see by referring to the diagram.

Alkaline and Earthy Phosphates in Urine.

Sp. gr.		Phosphate of Lime.	
In health soon after food:			
1027·3 to 1025·5		6·42 grs. to 7·96 grs. per 1000 grs. urine.	
Long after food:			
1022·8 to 1028·0		6·82 grs. to 8·85 grs.	„
In inflammation of the brain:			
12th day - -	1029·7	- - 13·15 grs.	„
14th day - -	1033·0	- - 12·11	„
16th day - -	1030·0	- - 9·53	„
18th day - -	died.		
Another case:			
13th day - -	1027·3	- - 11·13	„
16th day - -	1027·0	- - 11·04	„
16th night - -	died.		
In violent delirium, acute mania:			
	1029·3	- - 8·90	„
Convalescent:			
	1020·0	- - 3·11	„
Delirium with epilepsy:			
2nd day - -	1022·6	- - 9·96	„
3rd morning -	1024·8	- - 14·75	„
3rd night - -	1023·7	- - 14·38	„
4th night - -	1022·8	- - 12·10	„
25th night - -	died.		

I may mention, that these are averages of a multitude of experiments. Of course, knowing the quantity of sulphates and phosphates present, by an easy calculation you can arrive at the quantity of sulphuric and phosphoric acid in the urine.

Having thus got the quantity of the sulphuric and phosphoric acid in a state of health and at different periods, I can make experiments on the quantity which occurs in different diseases. If there be any diseases in which certain tissues of the body are undergoing more change than others, causing an excess of sulphates and phosphates to be thrown out of the body, I can determine the sulphates and phosphates present in the urine in those diseases, and thus arrive at the knowledge whether an excess of these salts in the urine is peculiar to those diseases. There is one tissue which especially contains phosphorus,—I mean the nervous tissue. We know that the nervous substance contains much phosphatic fat. Every particle of the brain, and every particle of every nerve contains phosphorus uncombined with oxygen. If in the brain an increased action is taking place—an increased destruction of substance—there will be an increased oxidation going on; and thus it is very clear that the matter thus thrown off, if it passes out of the body, should be found in the state of phosphorus oxygenized, that is, of phosphate. Thus, phosphates should occur in the urine in considerable excess when there is a rapid change taking place in the brain. But the brain not only contains phosphorus in large quantities, but sulphur also, which is present in the albuminous substance of the brain; and the sulphur is nearly equal to the quantity of *phosphorus present* in the phosphatic fat; and if there is a rapid oxidation of this *taking place, there* ought to be found the products of the change in the urine.

There is another tissue which contains little phosphorus, but a large quantity of sulphur—and that is the muscular tissue. If the muscular tissue, then, is undergoing excessive change—if there is excessive action taking place in it, it is reasonable to expect, if oxidation takes place in the body, that we should find an excess of sulphates existing in consequence in the urine.

Taking all kinds of diseases, such as they occur in the hospital, where the patients, for the most part, are allowed the same amount of exercise, or where they are almost entirely confined to bed and kept upon a diet of which we know the constituents, and thus we are therefore better able in the hospital than elsewhere to estimate and eliminate the effect of the diet and the exercise,—taking all kinds of disease, I find that, regarding the sulphates, there are two diseases in which there is an inordinate increase of these salts in the urine. These diseases are, first, acute chorea, in which there is an intensely rapid and most continuous action of the muscles; and, secondly, that restless disease which we know as delirium tremens. Cases of these diseases, extreme in intensity, have not yet occurred to me in sufficient number to establish the fact with that positiveness which can satisfy you or me; yet the experiments are sufficiently numerous to show the nature of the conclusions which must be drawn. In both these diseases, and in these alone, do I find the quantity of sulphates in the urine exceedingly increased; while, at the same time, the quantity of phosphates is not increased at all,—the total phosphates in some cases being even considerably lower than in a state of health. It is not in cases of slight chorea that this increase of the sulphates in the urine becomes apparent, but in cases in which the symptoms are most violent and uncontrollable, such as those which endanger life, and which sometimes occur, though but rarely, in our hospitals. In such cases I have never yet failed to find my conclusions established. For example, I found that in a little boy, eight years old, hardly more than one-third grown, and in whom I did not at all expect that the quantity of sulphates would approach to the quantity present in an adult—yet, in this boy, who was subject to most violent muscular action, and who from the spasmodic motions of his body could scarcely take a morsel of food into his mouth, but had to be fed almost by drops and crumbs, the urine, the specific gravity of which was 1030·6, contained an inordinate quantity of sulphates. There were 11 grs. of sulphate of baryta precipitated from every 1000 grs. of urine. As the disease began to abate, less and less of the sulphates were found to be present; and, ultimately, on the 102nd day, when he was on full diet, and was perfectly recovered, eating largely to make up for the exceeding loss he had sustained during his illness, the sulphate did not amount to the quantity present in the urine at the time when the disease was most violent, and when he was taking scarcely any food at all, but taking most energetic exercise, if I may so express it. The phosphates throughout were not increased at all. Another case which I may mention, is that of a girl, twenty-two years old, who also suffered from an acute attack of this disease. In this case, the sulphates were much increased; higher, indeed, than in any case I have ever met with, except in one instance of delirium tremens, in which there were 37 grs. of sulphate of baryta precipitated from every 1000 grs. of urine. In this case of delirium tremens there was an additional increase of the sulphates in the urine, in consequence of the administration of sulphate of magnesia as a medicine. It is found, by experiment, that when a salt containing sulphate ready formed is taken into the system, part of it at least passes off in the urine; this, no doubt, added somewhat to the very large amount of sulphates which I found in the urine. The patient, however, as is usual in such cases, was unable to eat anything, and was on the verge of death, for on the next night after the experiment was made, he died. In cases of chorea and delirium tremens I find not only that the quantity of sulphates is increased; but also the quantity of urea is far beyond the amount present in healthy urine. I found, that in every drop of the urine passed by these patients, there was so large excess of urea, that an immediate crystallization would take place on the addition of nitric acid, from the formation of urea. The same excess of urea which I have mentioned as occurring in these diseases is found also when persons are made to undergo violent exercise,—in the case of those who run for wagers, for instance,—their urine, when examined afterwards, is found to contain a great excess of urea. The cases mentioned in my Table are only a few out of very many which I have recorded in the 'Philo-

sophical Transactions' for last year, in which I have given a full Table of all the cases I had then examined. From these, you will see that there is only one class of diseases in which the amount of the sulphates alone is thus increased; that class is formed of those diseases in which the muscular tissue is continuously kept in most energetic action. It is only when the sulphur of the muscle is undergoing rapid change from oxidation that the sulphates are found in increased quantity in the urine. So also regarding the phosphates. I have said that there is no considerable amount of phosphorus in the muscular tissue, and that the phosphates are not, therefore, increased in those cases in which rapid change is taking place in the muscular tissue. But in the nervous tissue phosphorus exists in as large a quantity as sulphur; and in cases of inflammation of the brain, excessive delirium, and acute mania, the phosphates, as well as the sulphates, are found in increased quantities in the urine. I have examined hundreds of cases of different kinds of disease, and I find that in no other instances, except those of intense inflammation of the brain, intense delirium, and the like, do the phosphates increase to the amount given in my Table on the phosphates in the urine.

The total amount of phosphates in the urine of a healthy person, on full diet and good exercise, is between 6 and 9 grains, per 1000 grs. of urine, but the Table shows that the quantity is much greater in the diseases I have mentioned. In a severe case of inflammation of the brain (it is not impossible that even in slight cases, if the total quantity of urine secreted in twenty-four hours could be collected, an increase in the phosphates might be found), the amount was as high as 13·15 grs. in every 1000 grs. of urine, the specific gravity not being higher than in a state of health. In another case mentioned in the Table, the disease was at its height from the 13th to the 16th day; the specific gravity of the urine was about the same as in health, but the quantity of the phosphates was increased to 11 grs. in every 1000 grs. of urine. In a case of acute mania, where the specific gravity of the urine was 1029·3, the phosphates amounted to 8·90 grs. per 1000 grs. of urine; but, when the patient was convalescent, they were as low as 3·11 grs. per 1·000 grs. of urine. In a case of delirium, with epilepsy, which I met with in St. George's Hospital, the phosphates were still more increased. In the *post mortem* examination of this case, there was no positive evidence of inflammation of the brain, but I am not sure that it did not belong to that class of disease; as I cannot say that it was certainly a case of inflammation of the brain, I have named it delirium only. I come, then to the conclusion, that the sulphates and phosphates in the urine, are inordinately increased in cases where the tissues of the body, containing most sulphur and phosphorus, are undergoing most rapid and energetic action. These substances, having undergone combustion in the tissues, are thrown off by the urine, and, in passing out of the body, can be detected and weighed. As yet, there is no other method that I know of by which we can arrive at any knowledge regarding the oxidation of the sulphur and phosphorus of the tissues of the body. I am far from thinking, that the experiments which I have made on these subjects are sufficient for a perfectly safe generalisation. I am far from saying that a sufficient number of cases of each kind of disease have been examined; the experiments cannot be too often repeated. Enough has been done to show most distinctly the vast differences in the amount of sulphates and phosphates in different kinds of diseases, and to indicate the nature of the conclusions at which we shall ultimately arrive. At any rate, as far as we have gone already, we have very strong reason for believing, that where the tissues, which contain sulphur and phosphorus, are undergoing change, there a process of oxidation is taking place. I have shown you distinctly and decidedly, that nitrogen, when it is in combination with hydrogen, can undergo oxidation in the body; and I consider that it is no less certain, that the same oxidizing process takes place in the sulphur and phosphorus of the organic matter of which we are composed.

I pass on now to the mineral matters which exist in the body. If I take two portions of any organized substance, and heat one in a covered crucible.—that is, without free access of oxygen,—till it is charred, whilst I oxidize the other to the fullest degree, and then treat both with water and dilute hydrochloric acid, I get totally different solutions in the two cases. From the charred substance, I get much less saline matter dissolved out than from the fully oxidized portion; for, *when the substance* has been fully oxidized, almost all the alkalies, earths, metals, *phosphorus, and sulphur*, become soluble in water and acid. Professor H. Rose, of

Berlin, the first analytical chemist of our time, has devoted much attention to this subject. He concludes that in animal and vegetable substances, those mineral matters only can be said to pre-exist which can be extracted from the charred substance by water and acid; and on the contrary, those which can only be separated after perfect combustion (oxidation) belong to the original organic matter, as integral constituents in an unoxidized condition. Those alkalies, earths, metals, phosphorus, and sulphur, which are not soluble, must exist in the organised body in an unoxidized state, and in some peculiar combination. If they were oxidized, they must be dissolved by acid. An extensive investigation was then undertaken, to see whether any great difference could be traced in the quantity of oxidized and unoxidized mineral matters in various animal and vegetable substances. A translation of his papers is given in the 'Philosophical Magazine' for July, 1849. I have represented his results sufficiently for my purpose in this Table:—

Oxidation of 100 *Parts of the Inorganic Constituents.*

	Soluble in Water	In Acid	Not soluble until after complete combustion.
	A.	B.	C.
Ox-blood	60·90	6·04	33·06 (A. 30·4 salt.)
Horse-flesh	42·81	17·48	37·71
Cow's-milk	34·17	31·75	34·08
Yelk of eggs	40·95	8·05	51·00
White of eggs . . .	82·19	15·52	2·29
Ox-bile	99·85	4·93	4·22
Urine	90·87	8·54	0·55 (C. silica.)
Solid excrements . .	18·55	62·30	19·15 (C. 18·1 silica)

If I take milk, for instance, and char it, without the free admission of oxygen, 34·17 grs. in every 100 grs. of ash will be soluble in water, and 31·75 grs. will be soluble in dilute hydrochloric acid; 34·08 parts will remain insoluble in these re-agents; but if I oxidize the substance to the greatest degree, if I burn the milk with the freest access of oxygen, the whole of the ash will be soluble in water and hydrochloric acid. Professor Rose finds that the quantity of matter which is soluble in acid and water, after any animal substance has been charred, varies in different substances. In the charred ash of urine, no less than 99½ parts in every 100 are soluble in hydrochloric acid and water. In the charred ash of the yelk of eggs the same re-agents dissolve only 49 per cent. of the ash. What is the reason, he asks, why these substances were not soluble when they were charred, whilst, when burnt with free access of air, the whole of the ash became soluble in hydrochloric acid? Clearly because the constituents of the ash exist in a different state when charred from that in which they exist when fully oxidized. He finds, then, that in different substances, there are different proportions of oxidized and unoxidized ash. If the constituents of the ash are all soluble in water and hydrochloric acid, when only charred, he calls the substances which furnish such an ash, teleoxidic bodies. If the constituents of the ash, after charring, are not all soluble in water and hydrochloric acid, as in the case of milk, the ash of which is partly oxidized to the greatest degree, and partly unoxidized to the greatest degree, he calls substances which have an ash like milk, meroxidic bodies. Substances which, when charred, yielded nothing to water and hydrochloric acid (such substances, however, he has not yet met with), he would call anoxidic bodies. The vegetable creation, you will remember, in my first lecture, was stated to live by giving off oxygen, under the influence of light; Professor Rose, therefore, conjectured, that the ash in all vegetables would be in a state of imperfect oxidation,—that is, in a meroxidic state; possibly also some vegetable substances may be in an anoxidic state. He has found, by experiment, that no vegetable ever contains its mineral constituents in a teleoxidic state. In animal life, the action of oxygen never ceases. The substances which are first formed in the animal body from the nutritive vegetable substances should contain some meroxidic substances; that is, the mineral constituents of the chyle and of the blood, previous to complete combustion, should only be partially soluble in water and acids. Flesh being formed from the blood, should contain more oxidized inorganic salts, and less deoxidized salts, than the blood; whilst the excretions, having been fully exposed to the action of the

inspired oxygen, should give perfectly oxidized inorganic constituents. Thus, he finds, in substances like the urine and the bile, that the mineral constituents exist in a state oxidized to the greatest degree,—the ash being, when these substances are charred, almost entirely soluble in water and dilute hydrochloric acid; that is, neither the ash of the urine nor of the bile requires to be further oxidized out of the body to be made perfectly soluble. In the body the constituents of the ash have already undergone the greatest oxidation by the action of the oxygen which has been inspired. These are most interesting analyses, which we owe to this great analytical chemist; they tend still further to show that a process of oxidation is going on in the human body. Thus, in animals who take in oxygen, the food (as wheat, for example) is meroxidic going into the body, whilst the excretions (urine, for example) are teleoxidic coming out of the body. Whilst in vegetables that give off oxygen, their food (manure), as bones, urine, guano, is teleoxidic going in; their full development as grain gives meroxidic or anoxidic substances.

These experiments are a further confirmation of the action of oxygen in the body. Not only does the oxygen act on hydrogen and carbon, but on sulphur and phosphorus, and even the mineral matters themselves.

No sooner was oxygen discovered than the attempt was made to apply it as a medicine. The Pneumatic Institution, as it was called, was founded by Dr. Beddoes, for the purpose of using oxygen as a remedial agent for all kinds of disease; but, though it was supported by the energy and truthfulness of Dr. Beddoes, and by the sense and skill of James Watt, the great engineer, who applied himself with the greatest zeal to the carrying out of the mechanical processes of administering the oxygen to the patients; and though it called forth the talent of that great chemist to whom we are so largely indebted—Sir Humphrey Davy—yet it utterly failed in accomplishing the object for which it was established. No satisfactory conclusions were arrived at; no power was obtained of using the oxygen as a remedy; and the undertaking came to naught, not because the oxygen was impotent (for it is the most powerful agent in nature), but because the means of applying it advantageously were unknown; and they remain equally unknown now. By the inhalation of oxygen, it is beyond our power as yet to interfere in the chemical processes which are taking place in the body; we know no more now respecting the means of applying it as a remedy than was known in the time of Dr. Beddoes and Sir Humphrey Davy. It is only by carefully determining how far the means which promote oxidation out of the body affect the oxidizing processes in the body, that we can arrive at any certain knowledge as to the remedial use of this most potent agent. At present, all direct attempts to employ it as a remedy must be described as having altogether failed.

I shall, in concluding this lecture, endeavour to point out the object which is obtained in the body by this action of oxygen—by the process, in fact, of respiration. The first great object of the action of the oxygen is, without doubt, the removal of injurious substances; and the second, which is closely related to this, is the production of animal heat, whereby chemical changes are promoted; the nervous and muscular forces are enabled to act more readily; and nutrition is more perfectly performed.

The injurious substances which have to be removed, or must be counteracted in the body, are the products of the changes in the old tissues of the body, or they are poisonous substances taken by accident or as medicines. It is the property of the inspired oxygen to act upon these in such a manner as either to render them harmless, or to promote their removal from the body. Let me suppose that a dose of prussic acid is taken as a medicine, and not in sufficient quantity, therefore, to cause death; ultimately, what becomes of it?

See in this diagram what happens when prussic acid is burnt out of the body.

Prussic acid + 8 oxygen = water + carbonic acid + nitrous acid.

$$HC_2N + 8O = HO + 2CO_2 + NO_3$$

It can be oxidized, and be converted thereby into the comparatively harmless bodies—water, carbonic acid, and nitrous acid. Precisely the same change occurs when prussic acid is taken into the body; there also it is oxidized and destroyed. Or let me suppose, that a substance arising from the changing tissues is taken into the body. Let me take urea as an example. If I dissolve it in a little alcohol,

and burn it, the urea will be oxidized, and I shall have water, carbonic acid, and nitrous acid produced.

Urea + 12 oxygen = water + carbonic acid + nitrous acid

$C_2 N_2 H_4 O_2$ + O_{12} = 4 HO + 2 CO_2 + 2 NO_3

By the simplest experiment, I can show you the nitrous acid. This glass jar, the sides of which are moistened with starch, iodide of potassium, and dilute hydrochloric acid, is held over this alcoholic solution of urea which is burning in a watch-glass, and in a few moments the well-known re-action, as you see, appears. If the urea is taken into the body, and I have taken it in large doses, the same oxidizing action occurs,—the nitrous acid can be detected in the urine. The inspired oxygen acts on the urea, and it is decomposed.

The action of oxygen in the human body is not at all times and in all states uniform. This may be concluded from experiments on the variations in the temperature of the human body. Some accurate results from experiments on himself have been given us by Dr. Davy in a paper in the 'Philosophical Transactions' for 1845, p. 325.

Variations of Animal Heat.

		Pulse.	Internal Temperature.	External.
8 a.m.	—	58	98·7°	54·6°
12 night	—	55	97·9	62·4
Walking	—	84	highest 99·5	
Riding	—	52	97·7	

Mental excitement raised the temperature; a full meal depressed it.

At 8 a.m. he found the pulse 58; the internal temperature of the body, 98° 7; and the external temperature, that is, the temperature of the room, 54° 6. At 12 at night he found the pulse slower; this has also been observed by others to be the rule; and the internal temperature was lower, though the external temperature was higher. By active exercise the pulse rose to 84; and the highest internal temperature which he then gives (showing the more rapid oxidation produced by exercise) is 99·5. In passive exercise, when he was drawn through the air in a carriage, whereby a considerable loss of heat occurred, the pulse and the temperature both fell.

The following equation, which we owe to M. Baral, though in great measure conjectural as to the amounts stated, yet well shows the different ways in which the animal heat is consumed. He supposes the heat produced in the body by the action of oxygen to be represented by 100. How, then, is that heat lost? By evaporation of water and by the breath, 24·1; by expired air, the heated carbonic acid passing off, 7·3; by heating food, that is, by raising cold food to the temperature of the body, 2·2 (and you remember that a full meal depresses the temperature of the body); by evacuations, 1·8; by radiation and contact, that loss which we endeavour to prevent by clothing ourselves warmly, 64·6.

Comparison of Heat Gained and Lost.

Heat produced by Oxygen, 100	=	Heat lost. By evaporation of Water, 24·1	By expired Air, 7·3	By heating Food, 2·2	By evacuations, Urine, &c. 1·8	By radiation and contact. 64·6

Med. Times, Sept. 13, 1851, *p.* 270.

183.—*Use of Tannate of Alumina.*—At a meeting of the Medical Society of London, Mr. Rogers Harrison placed before the Society a specimen of Tannate of Alumina, and recommended its employment in the treatment of purulent and muco-purulent discharges from the urethra, especially when the former were not of an acutely inflammatory character.

Mr. Harrison had found the local exhibition of the remedy in question followed by the most satisfactory results. The method of using was to throw into the passage an injection containing from 2 to 10 grains of the salt dissolved in distilled water, the strength of the solution being in a great measure determined by the amount of smarting pain produced. The most advisable method was just to keep

the strength of the injection up to the smarting point. He thought it injurious to produce more than a gentle scalding.

Mr. Harrison did not anticipate, of course, equal success in every case, but he generally found the disordered condition of the urethral mucous membrane removed in the course of one or two weeks, in the ordinary run of cases.

On his recommendation, some of his professional friends had employed it in their practice, and from their reports he was supported in his high opinion of the remedial properties of tannate of alumina. The combination of alumina and tannic acid produced by Mr. Rodgers Harrison, was of a dirty yellowish colour, and in crystals about the size of those of coarse sugar, and readily soluble in hot water. —*Med. Gazette, Nov.* 14, 1851, *p.* 853.

184.—*Efficacy of the Tannate of Quinine.*—A report was read before the Academy of Medicine of Paris on the 17th Feb., 1852, by M. BOUVIER, in the name of a committee composed of MM. Orfila, Bussy, and himself, touching a paper of M. Barreswil on the therapeutic properties of tannate of quinine. The committee having instituted investigations on the subject, have come to the conclusion— 1. That the tannate of quinine is an anti-periodic agent. 2. That it possesses, the weight being the same, an activity equal but not superior to that of the officinal disulphate of quinine, for the cure of ague. 3. That it does not shield from relapse any more than the sulphate of quinine. 4. That it presents hardly any bitterness, which circumstance renders its administration easy, even with very delicate persons and children. 5. That by clinical experiments it has been proved that the tannate of quinine acts less on the organs of digestion, and especially on the nervous system, than the disulphate of quinine. 6. That whilst it bears analogy with the Peruvian bark by its constituent principles, and with the disulphate of quinine by the fixity of its composition, it is also very like these two substances by its therapeutical action. The reporter stated finally that the tannate of quinine acts in ague as the Peruvian bark or the disulphate of its alkaloid, and that it may in certain cases take the place of the latter substance, though it has the disadvantage, as an amorphous salt, of being very liable to adulteration. He also added that it should be borne in mind that the tannate has not been tried in pernicious intermittents. Thanks were voted to the author, and he was requested by the Academy to continue his investigations, and report their results to the Society.— *Lancet, Feb.* 28, 1852, *p.* 221.

185.—*A Comfort for Bed-ridden Patients.*—[MR. HOVELL adopted the following simple means in the case of a patient in the Hackney Union Infirmary, who had lost the use of the sphincters. They proved a great comfort both to the patient and surgeon. He says:]

Eleven days ago, I adopted the plan of placing beneath her a calico bag two feet square, partially filled with Irish peat-charcoal, so as to form a sort of cushion and absorbing medium. It has had the happy effect,—which continues even now, without any necessity for changing the charcoal,—of completely neutralizing all unpleasant odour; and if the bed becomes partially wet, all the offensive ingredients are absorbed and neutralized by the charcoal, which thus is a most simple means of remedying a great nuisance, and one that requires the most strict attention, at best, to prevent, and that attention is often difficult, and always expensive, to procure. In cases of incontinence of urine particularly, and, indeed, all attended with fœtid discharges,—cancer, compound fractures, &c., this plan, or some modification of it, might be adopted with advantage.

I have been informed that some of the same material has been placed in the urinals of the South Western Railway, with equally good result in the prevention of unpleasant odour; and that even after it has been unchanged for some weeks, the fluid that percolates has been found by chemical analysis to contain little or no trace of the organic or saline products of urine. This fact induced me to try it as *above.*

An argument in favour of its adoption in hospitals and lunatic asylums is, that

the peat, after its deodorizing properties are exhausted, becomes more valuable for the purpose of manure, so that its use is without expense.—*Lancet, Feb.* 7, 1852, *p.* 162.

186.—*New Mode of Preserving Pathological Specimens, by Suspending them in Antiseptic Gases.*—[Mr. W. B. RICHARDSON, at a meeting of the Medical Society of London, called its attention to this subject.]

In the early part of last session he had shown to the Society a portion of a heart that had been preserved for a long time in arsenuretted hydrogen gas. Owing to the poisonous nature of this gas, however, he had discontinued its use, and had since employed with great success either simple nitrogen or chloroform vapour, the antiseptic power of which had been shown by Robin and Dr. Snow. Four specimens were exhibited; the original one in arsenuretted hydrogen; two (portions of an ulcerated lung), one in nitrogen, the other in chloroform vapour; and an uterine tumour, in nitrogen. None of them had undergone the least change. The portions of lung had been in the bottles seven months. The mode of application was very simple. If nitrogen is used, the preparation is first fixed in the bottle; the bottle is next filled with water by a hole in the cork, and the gas introduced by displacement, under water. For chloroform, (the preparation being fixed,) a few drops are placed in the bottle whilst held in hot water, and when the vapour begins to pass over, the mouth of the bottle is closed. In both instances, the mouth of the bottle must be carefully sealed. He believed that dead matter was preserved in these gases merely by its being removed from the influence of oxygen, and he had no doubt that if pure nitrogen were employed in the way described, it would be found to surpass every other process for preserving flesh for dietetic purposes. —*Lancet, March* 27, 1852, *p.* 307.

187.—ON REINSCH'S PROCESS FOR THE DETECTION OF ARSENIC.

By DR. HARRY RAINY, Regius Professor of Forensic Medicine in the University of Glasgow.

This process consists in boiling the suspected fluid with about 1-10th of its bulk of muriatic acid, along with copper. The arsenic is deposited on the copper in the form of a steel-grey film.

It is generally supposed that this process is equally applicable to all the compounds of arsenic soluble in dilute muriatic acid; and that, in all circumstances, it detects the presence of the metal with a delicacy more than sufficient for every practical purpose.

Soon after the publication of Reinsch's method I made various experiments, with the view of determining the limits within which its indications might be relied on. The result was unsatisfactory, for while in some cases it seemed to be fully as delicate as the method of Marsh, in other cases I failed to obtain the metallic deposit where the arsenic was present in a much higher proportion. Similar observations have been made by others, for it is stated by Fresenius and Van Babo, that "the presence of nitrates and various salts of mercury and other metals renders the separation of arsenic by copper difficult or even impossible." It seems also to be a general opinion, that when the proportion of arsenic is extremely minute the process of Marsh is decidedly preferable.

It is obviously important that the cause of such discrepancies should be investigated, as the great rapidity and simplicity of Reinsch's process render it peculiarly suitable for medico-legal investigations, and give it a decided superiority over every other, if it can be conducted in a manner that will ensure equal delicacy.

The following experiments were made with the view of ascertaining the cause of these discrepancies, and, if possible, the means of preventing them. The copper was used in the form of very thin foil, which was easily cleaned and polished, so as readily to show any change of colour; the fluid usually contained *one-tenth* part by measure of muriatic acid of the ordinary strength, except when the object was to ascertain the effect of varying this proportion; and in order to

prevent any diminution of the fluid, or any change in its strength during the boiling, a condenser containing cold water was placed closely over the mouth of the vessel in which the process was carried on.

1. My first object was, to ascertain the *extent of copper surface* that can in the most favourable circumstances be distinctly coated with a given quantity of arsenic. The results were very uniform. *One thousandth of a grain* of arsenious acid gave a full steel colour to *one square inch* of copper surface. When *two* square inches of copper surface are used with the same quantity of arsenious acid, the effect is still distinct; but the deposit is in these circumstances so thin that there is a tinge of yellow, apparently from the copper shining through or not being uniformly coated. *Two* square inches of copper surface, then, is the utmost extent of copper surface that can be distinctly coated by *one-thousandth* of a grain of arsenious acid; and it can be proved that the thickness of the film of deposited metal, if consisting merely of arsenic, does not exceed 1-4,000,000th of an inch.*

It follows from this result, that if the extent of copper surface be too great relatively to the arsenic present, no distinct deposit will be obtained; thus, a fluid containing one-thousandth of a grain of arsenic with *three* square inches of copper surface, might give a tarnish, but no distinct coating.

2. The effect of *dilution* was next examined. When the fluid was to the arsenic as *one million* to one, the deposit was distinctly formed in fifteen to twenty minutes. Thus, one-thousandth of a grain of arsenious acid in one thousand grains of fluid, and consequently constituting one-millionth part, gave a distinct coating to one square inch of copper surface in twenty minutes. The same quantity of arsenious acid, in two thousand grains of fluid, also gave a deposit on the copper; but it was less distinct and required a longer time. It appears then, that with a dilution of one million times, the effect is distinct and prompt, and when the dilution is carried to two million times it is indistinct and tedious. A dilution of two million times appears to constitute the practical limit in Reinsch's process. By continued boiling it is easy, of course, to concentrate the fluid, so as to bring the dilution within these limits, if arsenic be present in any proportion, however small; for there appears to be no loss of arsenic by evaporation, during the boiling.

3. *The proportion of muriatic acid* in the solution has a considerable influence on the *rapidity* of the deposition, and even on its *production*, when the arsenic is in very minute quantity. Thus, if the arsenic is less than one-millionth, the process is very slow in a fluid containing one-tenth muriatic acid of the ordinary strength; but when it amounts to one-seventh or one-sixth, the deposition is much accelerated. And in solutions in which the quantity of arsenic is so small that with the ordinary proportion of acid no deposit is obtained, the copper becomes distinctly coated if the proportion of muriatic acid is doubled.

4. From these observations it would follow that the rapidity with which copper acquires a distinct arsenical coating is directly as the proportion of arsenious acid and also of muriatic acid in the solution, and inversely as the extent of the copper surface.

5. As copper receives a coating of a similar colour from other metals, from sulphur and sulphuretted compounds, the mere formation of such a deposit cannot be considered a conclusive proof of the presence of arsenic. It is merely a convenient method of *separating* the suspected substance, in order that it may be subjected to the appropriate tests. The most satisfactory of these tests are, the formation of a white crystalline sublimate by heating the coated copper; the solution of this sublimate in water; and its conversion, by the appropriate reagents, into arsenite of silver, orpiment, and arseniate of silver, all of which are very easily recognised by the peculiarities of their colour and other properties. In estimating the value of Reinsch's process, it is therefore necessary to ascertain, not only the smallest quantity and the utmost dilution under which it can be separated and distinctly exhibited on copper, but also the smallest quantity which, when so separated, can be satisfactorily subjected to the conclusive tests.

In repeated experiments I found that one-thousandth of a grain of arsenious acid in one million times its weight of fluid, could be separated as a distinct deposit

* *I afterwards found that the film contains a large proportion of copper, and consequently the thickness will be greater than is stated above.*

on copper. The copper thus coated, when heated gently in a small tube, yielded a slight but distinct sublimate, most obvious on a black ground, and which, with a magnifying power of ten to twenty diameters, was found to consist of crystals with triangular facettes, and which, when dissolved in water, yielded orpiment and the red arseniate of silver, when treated with the appropriate reagents.

This I believe to be as great a degree of delicacy as has actually been obtained by the more tedious and troublesome process of Marsh, and is more than sufficient for every practical purpose.

6. When investigating the delicacy of Reinsch's process, I prepared quantities of very dilute solutions of arsenious acid, varying at times from 1-10,000th to 1-100,000th, and kept these solutions in readiness for the experiments which I had planned. When first tried with copper and muriatic acid, they gave results entirely conformable to those already stated. A portion of any of these solutions, containing one-thousandth of a grain of arsenious acid, when diluted so that the fluid amounted to a million times the weight of the arsenic, gave a distinct and rapid deposit on the copper; but afterwards I could obtain no deposit from larger quantities of arsenious acid, though in a more concentrated state. As an example, a portion of solution containing 1-200th of a grain of arsenious acid in 200 grains of water, and consequently with a dilution of one in forty thousand, gave *no deposit whatever* when boiled in the ordinary way with copper and muriatic acid for upwards of fifteen minutes. In this case, the surface of the copper was only one-eighth of a square inch, and therefore could not interfere with the result by its too great extent.

I was perplexed with the apparent inconsistency of these results with those previously detailed, and began to suspect that I had been misled in my first estimate of the extreme delicacy of Reinsch's process. But after repeated trials, I found the difference to depend on the *length of time that the solution is kept. Very dilute solutions of arsenious acid become gradually less and less sensitive to Reinsch's process*, so that after several weeks no deposit can be obtained on copper from solutions containing arsenic in the proportion of *one* in *fifty thousand*, or even one in twenty thousand.

7. I was thus led to examine whether these dilute solutions underwent any appreciable change in their chemical properties, and found that with nitrate of silver they gave a greyish cloud; when concentrated by evaporation to a small bulk, the residual fluid *strongly reddened litmus*, and when evaporated to dryness, left a white stain, which did not sublime at a low red heat. This stain, re-dissolved in a few drops of water, formed a solution which *still strongly reddened litmus*, and which, on the addition of a strong solution of nitrate of silver, gave a *brick-red precipitate*.

These experiments clearly indicate the conversion of *arsenious acid* into *arsenic* acid.—*Lancet, April* 24, 1852, *p.* 397.

188.—*On the Employment of Sulphate of Zinc as an Antiseptic.*—M. Falcony states, as the result of his experimental researches, that sulphate of zinc is not only eminently antiseptic, preserving animal substances from decay, but that it actually arrests the progress of putrefaction which has once commenced. The injection of four or five quarts of the solution of this salt in water, through the arteries, suffices for the preservation of a human body in a state of perfect flexibility for upwards of forty days. Anatomical preparations thus made, will serve for dissection for a considerable period; the use of the solution not affecting the steel instruments employed. M. Falcony has also found, that preparations which have undergone change by maceration, resume their original character when immersed in a solution of sulphate of zinc.—*Med. Times and Gazette, Jan.* 31, 1852, *p.* 124.

A SYNOPSIS,

CONTAINING A SHORT ABSTRACT OF THE MOST PRACTICAL ARTICLES IN THE FOREGOING PAGES OF THIS VOLUME; AND SHOWING, AT A GLANCE, THE MOST IMPORTANT INDICATIONS OF TREATMENT PUBLISHED BY DIFFERENT WRITERS WITHIN THE LAST HALF-YEAR. (ARRANGED ALPHABETICALLY.)

DISEASES AFFECTING THE SYSTEM GENERALLY.

FEVER.—A good mode of estimating the strength of the pulse in fever and other asthenic states, is to cause the patient to sit up in bed, and compare the condition of the pulse in this semi-erect posture with its state in the horizontal position. (Dr. R. B. Todd, p. 26.)

Intermittent.—Dr. Pfeufer having stated that he had been very successful in treating ague by a single ten-grain dose of quinine, and allowing at the same time a generous diet—Dr. Meyer gives the following as the result of a trial in 28 cases, three tertians and twenty-five quartans:—that not only will the majority of simple intermittents yield to this plan, but also most cases of obstinate quartan, complicated with enlarged spleen and liver, when endemic influences do not maintain the disease, or have not induced such changes in the blood as have required time to overcome. (Dr. Meyer, p. 24.)

Typhoid.—The great secret of success in administering support to patients in this disease is to give a small quantity very frequently, and also not to give a variety of food. Give your stimulants early. The best are brandy or port wine, but give them separately; the stomach does not easily digest two kinds of stimulants. Give them also in small quantities and often. Chloric ether is a good medicinal stimulant. If the pulse strikes the finger in a 'vacillating' manner it is a good sign for stimulants. The strength of the heart's action is also a good sign, the second sound especially is a good indication. If diarrhœa be present we must check it by the use of such astringents as contain tannin, as the infusion or tincture of rhatany, catechu, matico, logwood, or enemata containing small quantities of laudanum. Counter-irritation is also useful. If there is hemorrhage give five minims of turpentine, and repeat it every three or four hours. Apply it also externally to the abdomen. (Dr. R. B. Todd, p. 30.)

Typhus.—In the event of congestion of any important organ taking place resort to dry cupping extensively, either alone or followed by a blister. If the large doses of quinine fail to relieve let an emetic be given; if after this the febrile exitement still continue, give a full dose of the liq. opii sedativus, with a few drops of nitric acid; this will frequently give signal relief, and enable the patient to resume the quinine with every prospect of success. After the first decided impression on the disease made by the quinine, support the strength by good beef-tea and a moderate allowance of wine. Purgatives, without some decided necessity, should be avoided. When the head continues much involved, a strong capsicum enema, a drachm of the powder to ten ounces of water, will often afford relief. (Dr. R. Dundas, p. 1.) (See also 'Retrospect,' vol. XXIV. p. 24.)

RHEUMATISM.—Dr. Babington had generally ordered three ounces of lemon-juice to be taken three times a day, but he has recently ordered as much as six ounces. He states that there is no remedy with which he is acquainted to be compared in value to lemon-juice in the treatment of rheumatism. In gout and chronic rheumatism its effects are far less obvious and uniform. He believes it will be found a valuable agent in inflammatory diseases generally. (Dr. Babington, p. 32.)

Acute.—In all cases where albumen and urea are vicarious, and where coma supervenes, evidently from the accumulation of the latter principle in the blood, colchicum will prove of great value. (Dr. J. M. Maclagan, p. 38.)

Articular.—In proportion as this disease approaches in its characters to gout we *may expect to be* successful with colchicum. Give twenty minims of the tincture

or wine every six hours until some relief is obtained, or a grain of the inspissated juice or of the acetic extract of colchicum every four hours. (Dr. J. M. Maclagan, p. 41.)

In gout and articular rheumatism cover the part with a layer of collodion, so as to preserve it from the atmosphere. (M. Latour, p. 325.)

AFFECTIONS OF THE NERVOUS SYSTEM.

CHOREA.—Gymnastic exercises have been recently employed in the Hôpital des Enfans, Paris, with the most marked success, commencing with the most simple movements. At the twentieth lesson they were exercised in wrestling, and afterwards in running. A marked improvement soon became manifested, their countenances becoming animated, their flesh firm, their voices stronger, their appetites keener and more regular; glandular swellings, which had resisted all treatment, were resolved, and fistulous sores, that had been open for years, closed up. (Dr. Sée, p. 75.)

Another valuable method of treating chorea is by means of the sulphureous baths, as devised by M. Baudelocque, fifty-eight rapid and decisive cures having been obtained in 65 cases. Thirty drachms of sulphuret of potash are added to each bath, which is applied at least one hour daily at a temperature of 91°. Where the cure is retarded, it ordinarily depends on the patient's powers being lowered by other remedies, or insufficient diet; upon irritation of the skin, induced by the bath, or upon acute irritation of the internal serous membranes—circumstances contra-indicating the baths while they continue. (Dr. Sée, p. 76.) See "*Hysteria.*")

CONVULSIONS, *Infantile.*—In a case where all other means had failed in affording relief, the child was placed for an hour under the influence of chloroform. During the hour, no convulsions occurred, but as soon as the influence was withdrawn, they immediately recurred. The remedy was again resorted to for four hours more. For an hour after ceasing the use of the chloroform, there were no convulsions; but at the end of that period, they again recurred. The child was now kept under its influence, without intermission, for 24 hours continuously, with the exception of being allowed to awaken eight or ten times during that period for the purpose of suction and nourishment. After this time there was subsequently no recurrence of the convulsions, and the child gradually and completely recovered. (Prof. Simpson, p. 72.)

EPILEPSY, *Renal.*—To prove the presence of urea in the blood, take the serum from a blister, evaporate to dryness over a water-bath, then add alcohol, which dissolves it out. Evaporate this alcoholic extract to dryness, and, after mixing with a little water, so as to make a spongy mass, add a few drops of nitric acid; if urea is present, characteristic crystals of nitrate of urea are formed, recognised either by the eye or the microscope. In those cases of epilepsy where there is evidence of irritating matter in the brain, use a treatment actively eliminatory. Shave the head and blister the scalp, apply mustard cataplasms to the back of the neck, and purge freely. Elaterium is one of the best purgatives, as it carries off a great amount of serum, which contains urea. Warm baths, or hot air baths, are very valuable where there is not much depression of the system. (Dr. R. B. Todd, p. 66.)

HYSTERIA.—Thirty drops of the tincture of colchicum, taken every eight hours, was followed by complete relief. In *chorea* also the same remedy has proved valuable. (Dr. J. M. Maclagan, p. 300.)

LEAD PALSY.—The state of lead cachexia is very greatly improved by the long-continued use of sulphur baths. (Dr. R. B. Todd. p. 69.)

NEURALGIA.—See "*Sciatica.*"

PARALYSIS *of the Lower Limbs.*—Give the ergot of rye in daily doses of from 8 to 40 grains. (M. Gerard, p. 59.)

Rheumatic and Local.—Pass galvanic currents along the parts affected by means of a Pulvermacher's chain battery. (Mr. T. J. Vallance, p. 57.)

SCIATICA.—Sciatica, not connected with mechanical causes, as tumours, accumulation of fæces, &c., may be cured in fourteen days, by rubbing along the affected

nerve, from above downwards, ℥ss of veratria ointment (gr. v. to ʒss), every night at bed-time. The friction to be performed with a horse-hair glove until severe tingling is produced. (Mr. C. R. Bree, p. 74.)

Sciatica and Neuralgia.—Some cases recorded by Dr. Belcombe were entirely cured by the insertion of a needle just at the seat of pain, to some depth, and another two inches lower in the same direction. These were kept in for two hours, and then withdrawn. In cases of *sciatic lumbago*, give acetate of potash in compound infusion of senna, the patients finding much relief from the movement of the lower bowels, and pressure being taken off the kidneys. In the *neuralgia* of the upper extremity, carbonate of iron in full doses was given. (Dr. H. S. Belcombe, p. 74.)

AFFECTIONS OF THE CIRCULATORY ORGANS.

Aneurism.—Displacement of the fibrine in the clot of an aneurism sometimes happens with consequent interruption to the current of blood, and sometimes spontaneous cure. Mr. Fergusson, in imitation of this process, by manipulation, dislodged a portion of fibrine in the case of an aneurism of the subclavian, with the effects of instantaneously stopping all pulsation of the upper limb. In four days a slight pulsation was perceptible at the wrist, but all pulsation had ceased in the axillary. The tumour was diminished considerably in size, and became firmer to the touch, so that there is ground to hope it may be successful. (Prof. Fergusson, p. 193.)

AFFECTIONS OF THE RESPIRATORY ORGANS.

Cough *in Phthisis.*—(See "*Phthisis.*")

Inspiration, *Wavy*—In this symptom, the inspiratory sound, instead of being equably sustained from its commencement to its termination, is divided into several parts. The sensation conveyed to the ear suggests the idea of some obstruction to the pulmonary tissue, yielding at gradual intervals to the admission of air. The most likely explanation of this kind of sound seems to be the existence of an exudation less albuminous than tubercle, less fibrinous than the usual product of fibrinous inflammation, about the walls of the small bronchial tubes and the interstices of the pulmonary substance calculated to impair the elasticity of the surrounding structures. In treating this symptom, we must promote healthy nutrition, correct hepatic congestion as much as possible, select climate so as to secure, without unseasonable exposure, much easy exercise in the open air. Iodide of potassium and iodide of iron are appropriate remedies. Counter-irritation is also sometimes useful. (Dr. T. Thompson, p. 103.)

Lungs, *Affections of.*—When the period of acute inflammation has subsided in broncho-pneumonia, pneumo-bronchitis, chronic-bronchitis, pituitous catarrh, and in dry asthma, iodide of potassium is an exceedingly valuable remedy, although the theory of its beneficial influence is somewhat obscure. (Mr. R. Molloy, p. 108.)

Phthisis.—The peculiarity of phthisis is, that an excess of acidity exists in the alimentary canal, whereby the albuminous constituents of the food are rendered easily soluble; whilst the alkaline secretions of the saliva, and the pancreatic juice, are more than neutralized, and rendered incapable either of transforming the carbonaceous constituents of vegetable food into oil, or of so preparing fatty matters introduced into the system, as will render them easily assimilable. In consequence, more albuminous than fatty matters enter the blood, and the necessary waste of structure is supplied by the absorption of the adipose tissues of the body. Hence the emaciation which characterizes the disease. In the meanwhile the lungs become especially liable to local congestion, leading to exudations of an albuminous kind, which is tubercle. This, in its turn, being deficient in its necessary proportion of fatty matter, elementary molecules are not formed so as to constitute nuclei *capable of further development* into cells: they, therefore, remain abortive, and *constitute tubercle corpuscles.* (Prof. Bennett, p. 97.)

One of the earliest symptoms in phthisis is a modification of the expiratory murmur, consisting in an apparent prolongation. With the progress of the disease, the duration of the inspiratory murmur usually lessens materially, though not necessarily in proportion to the prolongation of the expiratory. The expiratory murmur may, as the disease advances, gradually increase, until, instead of occupying, as in the natural state, a fourth of the period of healthy inspiration, it may even come to exceed in duration the inspiratory murmur. Many cases have proved the accuracy of this incipient symptom. When the expiratory murmur is heard extensively, or on both sides, unassociated with bronchitis, emphysema, or condensed lungs, there is great reason to fear not only that the disease is phthisical, but that it will make rapid progress. When the change of murmur is limited to a small portion of lung, and the general condition of the patient is favourable, the evidence of the phthisical disease is fully as conclusive; but by careful regulation of diet, by securing exercise in the open air, by promoting healthy nutrition, and administering suitable remedies, such as iodine, iron, solution of potash, and cod-liver oil, more decided mischief may be averted. (Dr. T. Thompson, p. 99.)

Cough in Phthisis.—The following remedies are exceedingly valuable in relieving the cough in phthisis. Hydrocyanic acid is an excellent remedy; or a draught, containing one-eighth of a grain of cyanide of potassium, an ounce of aniseed water, and some syrup of lemons, is perhaps more trustworthy than the usual form of administering prussic acid. We are, however, often obliged to place our chief reliance in opium, or the salts of morphia. A linctus may be made, composed of an ounce of conserve of roses, half an ounce of lemon juice, and half an ounce of syrup of poppies, or a drachm of tincture of opium and a drachm of diluted sulphuric acid, may be mixed with an ounce and a half of treacle or honey, and a teaspoonful given occasionally. An inhalation, from an infusion of an ounce and a half of the strobiles of hop, in a pint of hot water, sometimes proves very soothing; or an inhalation of two grains of the extract of opium. Few remedies, however, act so promptly and satisfactorily in allaying cough as the inhalation of 15 or 20 minims of chloroform. (Dr. T. Thompson, p. 106.)

Hepatic Congestion in.—Give a combination of chalybeates and saline aperients. The natural springs may be imitated by administering two grains of sulphate of iron, a drachm of sulphate of soda, a scruple of carbonate of soda, and ten grains of dinner salt, in half a pint or a pint of warm water, every morning. Use at the same time exercise in the open air, the shower bath, and friction of the skin. (Dr. T. Thompson, p. 104.)

AFFECTIONS OF THE DIGESTIVE ORGANS.

Ascarides.—Give kousso, both in the form of injections and by the mouth. (Dr. Hannon, p. 118.)

Diarrhœa.—Give sulphuric acid, in 20 minim doses, in an ounce and a half of water every hour. (Dr. H. W. Fuller, p. 119.)

Give dilute sulphuric acid in doses of 20 or 30 minims rapidly repeated, if the purging is repeated or pain comes on. (Mr. T. Buxton, p. 121.)

Creasote is very valuable in cases of diarrhœa, not arising from foreign matter in the intestines, or in diarrhœa following the administration of purgatives, to remove these foreign matters; or where, after an acute diarrhœa, a passive state of purging exists. It may be prescribed in doses of two minims, with half a drachm of sulphuric ether, repeated every one or two hours. In children, to secure success, the dose must be small: one-fourth, one-sixth, or one-eighth of a drop is sufficient for children under two years old; to adults two or three drops may be given. (Dr. B. W. Richardson, p. 315.)

Hemorrhoids.—These depending upon a separation of the mucous membrane from the other coats of the bowel, the object is to unite this with the other coats so as to prevent its prolapsion. The adhesive process to attain this end may be excited by the removal either of some of the hemorrhoids, or of a portion of the skin and mucous membrane at the margin of the anus. The effect of this operation is the effusion of lymph, the formation of a cicatrix, and, as a consequence of the process, the agglutination of the parts together. The removal of the hemorrhoids

is the more direct and preferable method. To arrest the hemorrhage, we must rely upon the replacement of the vascular growth within the sphincter, and the maintenance of the horizontal position of the body. If the loss of blood, however, be considerable, the operation for affording permanent relief must be performed. The ligature used is of thick silk, and is carried double through the middle of the hemorrhoid, or of the portion of this to be acted upon by means of a needle furnished with a handle, or even with a common bent needle. Each part of the ligature is to be tied at the side, and as firmly as possible, so as to destroy the vitality of the included hemorrhoid. Caution is necessary that none of the skin be included in the ligature; and should any happen to be within the space to be acted upon, it must be divided in the track of the ligature before this is tied. The efforts of the patient are frequently necessary to keep the part properly within reach, hence it is not advisable to administer chloroform, as the parts are at once generally withdrawn within the sphincter. If the bowels do not act within three or four days, an enema or mild aperient may be administered. If there is difficulty of evacuating the urine, hot fomentations, and a dose of opium, will expedite the removal of this distressing incommodity. It is important in all cases to avoid applying ligatures in large numbers, especially where the patient is in impaired health, or debilitated by loss of blood. The best plan is to limit the number to three, and even to have recourse to a second operation, than to do too much even once in a life. (Mr. R. Quain, p. 208.)

The hemorrhage from the operation may be controlled in the usual manner by tying at the time the small arteries that bleed. This method cannot be resorted to some hours after the operation if secondary hemorrhage should come on; therefore, it is better to insert a pin into the bleeding part and control it by a few turns of the ligatures. The pin should be one like the hare-lip pin formerly used—blunt at both ends, having a moveable point and a hole or eye at one end to insert a thread—useful in withdrawing the pin. The pin may be withdrawn in from twenty-four to forty-eight hours. (Mr. R. Quain, p. 212.)

Hernia, *Strangulated.*—Chloroform administered in the usual manner is an exceedingly valuable remedy, superseding bleeding, tobacco, &c., and should be tried along with the taxis before an operation is resorted to. (Mr. E. Stanley, p. 205.)

The following plan was resorted to before operation in a case under Dr. Wise. The patient was placed upon the table and a long sheet folded several times on itself carried round the lower part of the abdomen of the patient, was twisted on itself in front and again on the sides, so as to enable an assistant standing on each side of the patient to hold the extremities of the sheet and to pull the bowels gently upwards or towards the patient's head, while a third assistant held the feet steady and the surgeon used the taxis. The plan succeeded perfectly, and may be safely resorted to in an early period of the hernia. (Dr. T. A. Wise, p. 206.)

Intestines, *Stricture of.*—If the stricture is low in the colon, the vomiting does not come on until some time afterwards; if it is in the small intestines the vomiting comes on early. If the vomiting is not stercoraceous it is not likely to be an obstruction of the small intestines. When it is in the larger intestines it takes a longer time to produce stercoraceous vomiting than when it is in the small. When the seat of obstruction is below that point, Amussat's operation of an artificial opening is the most preferable in the left colon. When this, from the circumstances of the case, cannot be effected, then the opening should be made in the right colon. (Mr. C. H. Hawkins, Mr. Norman, Mr. Hodgson, p. 204.)

Peritonitis.—Apply a layer of collodion over the whole surface of the abdomen. (M. Latour, p. 325.)

Rectum.—*Inflammation and Ulceration of.*—Make a decoction of tormentilla. Boil three ounces of the root in a pint and a half of boiling water down to a pint. Let four ounces of this decoction be thrown up the rectum twice a day, and be retained a quarter of an hour. A little castor-oil should be given to keep the bowels loose. (Mr. W. Coulson, p. 213.)

Tænia.—Give a powder at bed-time containing four grains of calomel and two of ipecacuan, and a strong draught of concentrated decoction of aloes, and in the *morning give a drachm* of oil of male-fern. (Mr. R. Molloy, p. 118.)

Tænia Solium.—The dose of kousso to be given must be decided by the

idiosyncrasy of the patient. At all events it should be given in a good large dose to produce a decided effect; kousso is also liable to be greatly adulterated, which likewise renders the dose uncertain. A good way of preparing it, however, is to macerate it in hot water (not boiling) for three hours, and it must then be taken entire, without being strained, after a previous fast of several hours. (Mr. J. Vaughan, p. 118.)

AFFECTIONS OF THE URINARY ORGANS.

Bright's Disease.—Urea in this disease accumulating in the blood, is best eliminated by the administration of colchicum. (Dr. J. M. Maclagan, p. 142.)

Diabetes.—Alkalies have proved more valuable than any other kind of treatment. At the same time substances should be given which cannot be converted into sugar, especially fatty substances, which can be used in the process of respiration. Alkalies have more power in those cases of stout subjects which bear some relation to gout, than in the more severe and confirmed cases where there is great emaciation. (Dr. Bence Jones, p. 155.)

The principal indication being to prevent the elaboration of the saccharine poison in the stomach, and to restore defective digestion, Mr. Hogg endeavoured to find some medicine which would effectually arrest the saccharine fermentation. This he tried to effect with the sulphuret of arsenic, which was prevented being converted into a soluble sulphuret by combining the liq. arsenicalis with the hydrosulphuret of ammonia, the following being the form: liq. arsenicalis, 3ij.; hydrosulphuret of ammonia, ℥xx.; tinct. henbane and lavendar, each 3ij.; infusion of buchu, ℥viij. In two cases in which this remedy was applied it was successful. (Mr. J. Hogg, p. 159.)

Hydrocele.—In the adult, first simply tap and draw off the fluid, and do not employ an injection. If the disease returns, then inject two drachms of one part of tincture of iodine to ℥j. of water. This is generally successful. If this fail, Mr. Cooper used to pass a seton through the hydrocele. The water was first drawn off, and then a long needle was passed with a curved point, and armed with a single thread of silk up the canula, bringing it through the skin of the tumour at about its centre, leaving the thread in situ, loosely tying together the two ends. When the inflammation commenced, he withdrew the seton. But this measure at the best is a very hazardous proceeding. Mr. Key, in 1837, passed a seton through a hydrocele, but without drawing off the fluid, and the patient died in four days after from excessive inflammation. In the hydrocele of children, the following lotion is valuable: ammon. muriat., 3j.; sp. vini rect., liq. ammon. acet., aa. ℥ij.; aquæ ℥iv. M. fiat lotio. (Mr. B. Cooper, p. 247.)

Lithotomy.—In the operation of Mr. Fergusson for stone, the peculiarity is that he makes the smallest possible opening in the neck of the bladder, so as just to admit the forceps into it. Just when the blades of the forceps enter the bladder, a gush of urine takes place, at which instant he opens them, and by a slight twist of the wrist the stone is caught at once, when with the utmost care it is gradually drawn downwards until the tissues yield and it is removed. Some advise free incisions, but Mr. Fergusson only notches the prostate. (Prof. Ferguson, p. 218.)

Spermatorrhœa.—Use quinine in the following manner: Quin. disulph. gr. vi.; acid. sulph. dil. 3j.; tinct. cardam. co. 3iij; aq. cinnam. ℥vss. M. sumat cochl. duo ampl. bis die. In lieu of sea-water, take a piece of common salt the size of a walnut, and dissolve it in half a basin of water. The scrotum and perinæum to be bathed with this by the aid of a sponge for five minutes every morning. A walk should generally be taken early in the morning and the last thing at night. The less sleep the patient has the sounder it will be. The best remedy for checking the erections is the remedy for chordee, a tea-spoonful of spirit of camphor in water. The bowels should be kept loose by the blue pill with rhubarb. If these means do not suffice to cure the disease, blister the penis in the manner recommended for gonorrhœa in the last volume of the 'Retrospect.' When self-pollution is indulged in, apply some irritant ointment to the penis, as the ung. ant. tart. once a week. (Mr. J. L. Milton, p. 241.)

Apply a solution of the nitrate of silver to the urethra. This may be made of

various strengths from one grain to the ounce up to two drachms. Dr. Thompson describes an instrument for this purpose at p. 244. In cases of gleet of an obstinate character, apply a solution of nitrate of silver, one or two scruples to the ounce, to the prostatic portion of the urethra. (Dr. H. Thompson, p. 242.)

Apply caustic locally, and exhibit conium and soda in the infusion of gentian, cold bathing, fresh air, and almost entire abstinence from alcoholic fluids. After the secretions of the patient have been got into good order, give small doses of the tincture of cantharides and the sesquichloride of iron in a bitter infusion. (Mr. J. Adams, p. 216.)

SUGAR.—*Tests for.*—Test a portion of urine with a solution of sulphate copper and liq. potassæ; a blue solution is first produced, heat it, and the blue colour becomes slightly yellow on the surface, and rapidly changes into that of a reddish yellow precipitate, which is the mixture of the colour of the urine and the colour of the sub-oxide of copper. With this test, independent of sugar, a large quantity of animal matter or of urea will produce a blue colour, but when heated the reduction of the copper does not take place near so rapidly.—A salt of another metal may be used instead of the copper, as silver with a drop of ammonia. The sugar takes the oxygen from the silver, and it is deposited in a beautiful metallic form on the glass.—Another easy test is allowing a drop of diabetic urine to dry on the glass, and examining it by the microscope, when remarkable tufts of stellated crystals will be seen. (Dr. Bence Jones, p. 155.)

New Test for.—To any liquid suspected of containing sugar, add a little carbonate of soda and a small quantity of magisterium bismuthi, and boil briskly; when the liquid cools, the bismuth, if sugar be present, is reduced, and forms a black powder. (Prof. Böttcher, p. 160.)

URETHRA, *Morbid Vascularity of.*—Apply strong nitric acid in the same manner as applied to hemorrhoids. It may be applied on a small rod of glass, which should be held to the part for about a minute, care being taken that each enlarged portion of the vessels is completely destroyed; in three or four minutes the pain ceases. (Dr. G. T. Gream, p. 297.)

URINE, *Deficiency of Urea in.*—In all cases where the renal secretion is deficient in urea and uric acid, these existing in the blood, give colchicum. (Dr. J. M. Maclagan, p. 38.)

Retention of, from Stricture.—Mr. Simon advocates a modified perineal operation in certain cases. He opens the urethra by puncturing a very small incision immediately in front of the prostate gland. He then runs a short elastic catheter along this wound into the bladder. He then leaves the stricture untouched for ten days more or less, during which the urine flows through the perineal catheter; at the end of that time, the stricture is sufficiently relaxed to begin its dilatation with a middle-sized instrument. (Mr. J. Simon, p. 222.)

AFFECTIONS OF THE SKIN, &c.

ACNE ROSACEA.—Dust the affected regions night and morning with the following powder: white oxide of zinc, two drachms; powdered starch, four ounces. (M. Cazenave, p. 138.)

ECZEMA, *Acute.*—Use starch powder as recommended by M. Cazenave. (See "*Acne Rosacea.*")

HERPES.—Use starch powder. (See "*Acne Rosacea.*")

IMPETIGO.—Use starch powder. (See "*Acne Rosacea.*")

ITCH.—In the Hospital of St. Louis, Paris, the itch is cured in two hours. The patient is first put into a warm bath, and rubbed for an hour with yellow soap. He then passes into a clean bath, where he continues to cleanse his skin for another hour. After leaving this, one of his fellow-sufferers rubs him over for half an hour with the following ointment:—Axunge eight parts, flowers of sulphur two parts, and carbonate of potash one part. The patient is then examined, and sent away cured, though sometimes pretty numerous vesicles on the hands and *elsewhere remain* unaltered. (Dr. Bazin and Dr. Hardy, p. 265.)

LEPRA.—Give wood or Archangel tar, in doses of fifteen minims in a capsule

three times a day. The patches may be also washed over with the tar, and the patient should lie in tar sheets. The tar may be washed off at intervals to see the progress that is being made. It is important that the capsules should be made of some soluble material, else they will pass through the bowels unchanged. (Dr. R. B. Todd, p. 264.)

Lepra and Psoriasis.—Give the sesquicarbonate of ammonia in the following manner. Sesquicarbonate of ammonia 3ss.; diaphoretic syrup ℥vij.; take one to three tablespoonfuls per diem. If diarrhœa, lassitude, cephalalgia, and rapid alternations of heat and cold were to occur the remedy must be suspended. (M. Cazenave, p. 264.)

Nævus.—In a recent case of nævus in a child, Mr. Hilton, by means of a thin wire heated by Cruikshanks' battery, removed it, cutting and searing it at the same time. The tumour was removed very quickly, with little or no hemorrhage. (Mr. J. Hilton, p. 195.)

[Another case of nævus cured by being incised with platinum wire heated red-hot by galvanism is recorded by Mr. R. M. Bernard, at p. 252.]

[For a new method of applying a ligature for the removal of a large pulsating nævus of the scalp by Mr. T. B. Curling, see p. 195.]

Prurigo.—Give 3ss. of vinum colchici thrice daily. (Dr. Elliotson, p 264.)

In pruriginous affections use powdered starch simply dusted over the part or applied after the latter has been cleansed by an alkaline solution and thoroughly dried. (M. Cazenave, p. 266.)

In prurigo of the genital organs, the groin, or the axilla, use the following powder: white oxide of zinc, two drachms; camphor, half a drachm; powdered starch, four ounces. (M. Cazenave, p. 266.)

In prurigo of the genital organs, first foment until the indurations are removed, then rub the affected part with an ointment made with two drachms of calomel to one ounce of axunge, and after each application dredge with a powder made with four parts of starch to one of finely powdered camphor. (M. Tournié, p. 247.]

Psoriasis.—See "*Lepra.*"

Scalp, *Eruptive Diseases of the.*—These are mainly: eczema, dry and moist; herpes; impetigo; pityriasis; which are inflammatory and non-contagious; and porrigo, which is non-inflammatory and contagious. As a general rule in the treatment of all these diseases, the hair should be cut close with a pair of scissors, and the head be covered with an oil-skin cap. An ointment must be made of the carbonate of soda from twenty grains to one drachm to one ounce of lard, or a lotion made of 3ss. to 3iss. to a pint of rose water. Potash may be used instead of soda in chronic cases. The ointment should be applied three times a day, smeared over the eruption, and washed off each morning with the corresponding lotion. Where crusts or scales are found, apply a linseed meal poultice for twelve hours, then the ointment for the same period; then remove the scales by gently washing it with the lotion. If the ointment disagrees the lotion may be used instead, but more frequently. In the chronic cases use 3ss. to 3j of citrine ointment to ℥j. of prepared lard; apply this at bed-time and use the lotion during the day. As an alterative, give to a child six years old gr. ss. of the protiodide of mercury, gr. ij. of mercury with chalk, and gr. ij. of aromatic powder, every second morning. During the treatment the child should be kept strictly on milk diet (Dr. Neligan and Mr. C. Poole, p. 265.)

Ulcers.—Exclude the atmospheric air by the following treatment, after the inflammation has subsided. From a piece of adhesive plaster somewhat larger than the sore, a portion, just the size of the sore itself, is cut out; the plaster is then applied to the part, and painted with collodion. Oiled silk is now placed over the ulcer, and made to adhere to the plaster by means of the collodion, by which process the air is completely excluded from the ulcerative surface. The whole is then secured by strips of adhesive plaster placed crosswise, and by a roller running from the toes to above the knee. (Mr. B. Holt, p. 266.)

Urticaria.—In a case of this disease the urea and uric acid being deficient, colchicum was employed with complete success, the urea being more than tripled in its amount, and raised above the normal standard. (Dr. J. M. Maclagan, p. 268.)

AFFECTIONS OF THE BONES AND JOINTS.

FRACTURE *of the Ribs.*—The two points of mechanical treatment in fracture of the ribs are, first, rest; and second, bandaging the chest, by fixing straps of adhesive plaster, circularly from the spine to a little beyond the sternum, on the injured side only, leaving the other free, as well as the abdomen on both sides. (Mr. J. Hilton, p. 177.)

JOINTS, *Diseased.*—That as the separation of diseased cartilages from the extremity of the bone is commonly by a process of 'shedding'—that as the portions so exfoliated tend to keep up a constant irritation within the joint—that as the natural outlets for these portions, the sinuses, are inadequate for that purpose—that as for their removal they have to be dissolved in the discharges of the joint, which is necessarily a very slow process, but as whenever they are removed an immediate process of reparation commences, Mr. Gay recommends that free and deep incisions should be made along the side of the joint, so as to lay open its cavity freely, and to allow of no discharges being retained by any possibility within it. The incisions should be made in the long axis of the limb. They should extend into the abscesses of the soft parts so as to lay them open, and the incisions should pass through sinuses, unless out of the way of the incision. If either of the bones be carious or necrosed, the incision should be made deep into them, to allow the free escape of diseased portions. Important vessels should be avoided. The wounds should be kept open by pledgets of lint, and free suppuration encouraged. (Mr. J. Gay, p. 179.)

THUMB, *Dislocation of.*—1st. Use a sufficient amount of traction (with Charrière's forceps) on the luxated bone in the direction of the axis of the thumb. 2nd. Push back with the operating thumb or left index finger, the head of the metacarpal bone, and keep it quite steady, so that when flexion is made, the head may not be thrust further into the palm of the hand. 3rd. When extension is carried to a sufficient extent, rotation either towards the outer or inner side should be made, so that the metacarpal bone may be freed from the muscular loop which is keeping it strangulated. This being done, the traction should be continued until the posterior part of the first phalanx has reached the level of the head of the metacarpal bone. Flexion is then to be made, whilst at the same time the left thumb of the operator forces backwards the displaced part of the first metacarpal bone. (M. Roux, p. 187.)

VENEREAL AFFECTIONS.

BUBO, *Opening of by Multiple Punctures.*—The bubo should be shaved. If the abscess is recent, and suppuration not extensive, one puncture may be made at the fluctuating point. If other glands suppurate they must be punctured in the same manner. If the collection is extensive and superficial, several punctures are required, not in the fluctuating centre, but at the circumference, by a straight bistoury passing subcutaneously to the centre; thus the skin is divided where it is adherent, intact, and possessed of its vitality. The bubo must not be pressed for two days, when it will gradually discharge itself. Sometimes the punctures close before the matter is evacuated; this may be prevented by gently pressing the abscess once a day. If they all close, it is better to allow them to do so than introduce tents, making one or two new punctures if required. By this plan not the slightest mark, or only a very slight one indeed, is left. (M. Vidal, p. 269.)

CHANCRE.—If the chancre is in an early stage, destroy it with nitrate of silver, or a small point of caustic potash, then dress the sore with a weak solution of chloride of soda and opium, over which a layer of finely carded cotton should be placed, in order to obviate the possibility of friction. Mercury may be resorted to with benefit when the regenerative process commences. If the sore is in an irritable and inflamed state, mercury should be avoided. If ulceration or sloughing *should come on*, we must trust to large doses of opium and iron. The simple *treatment without* mercury should on no account be risked where there is a ten-

dency to induration and an indisposition to heal. Where we are uncertain when constitutional syphilis will follow a sore, the wisest course is to avoid extremes by administering steadily moderate doses of blue pill, combined with Dover's powder, until the sore is healed, and no induration remains. (Mr. G. B. Childs, p. 267.)

COPAIVA, *Mode of Administering Balsam of.*—Thirty parts of the balsam are stirred round in a glass mortar with four of sulphuric acid. The mass quickly solidifies, and may be made into pills, which may be afterwards covered with a coating of gum and sugar. If the copaiva be adulterated with castor oil, the solidification does not take place; while, if adulterated with turpentine, although solidification does take place, the mass, when placed in water, becomes covered with a white, bitter resinous substance. The medicinal virtues of the copaiva are by no means impaired by the above proceeding, while its disagreeable flavour is destroyed. (M. Chervet, *Brit. and For. Medical Review*, Jan. 1852, p. 277.

GLEET.—As this frequently depends on an increased and altered secretion of the follicles of the prostate gland, it cannot be got rid of by astringent injections, as it is often impossible to reach the true seat of the disease: whereas the exhibition of chia turpentine, in doses of five grains, often puts a stop to the discharge instantly. (Mr. J. Adams, p. 214.)

Use an injection of tannate of alumina, five grains to the oz. of distilled water, before going to bed. (Mr. C. H. R. Harrison.—*Med. Times*, Nov. 15, 1851, p. 522.)

GONORRHŒA.—Give a saline purge during the first day, consisting of a seidlitz powder, with half an ounce of sulphate of magnesia; the recumbent position being enjoined, weak linseed tea with a little nitrate or bitartrate of potash, used as a common drink; and pure cold water used as an injection twice every half hour. During the second day the same quiet and rest observed, and drink used; a solution of two grains of sulphate of zinc in one ounce of water, is to be used as an injection twice every half hour. On the third day the rest may not be so strictly enforced, but the injection and drink must be used as before. The disease may be thus almost always cured in three days, but the success mainly depends on the persevering use of the injections. (Dr. P. Niddrie, p 270.)

Use an injection, before going to bed, of tannate of alumina, five grains to the ounce of distilled water. (Mr. C. H. R. Harrison,—*Med. Times*, Nov. 15, 1851, p. 522.)

MERCURY, *Substitute for, in Syphilis.*—Divide 15 grains of the bichromate of potash into 80 pills, with extract of gentian; one to be taken night and morning. (M. Robin, p. 268.)

AFFECTIONS OF THE EYE AND EAR.

CORNEA, *Iron Spiculæ in.*—Apply a solution of sulphate of copper to the eye, of the strength of one to three grains to the ounce. (Dr. H. Jeanneret, p. 253.)

OTORRHŒA, *with Perforation.*—In the muco-purulent discharge which pours out in otorrhœa, if globules of air can be distinguished in it, it is a positive and unfailing evidence of an opening existing in the membrana tympani. (Mr. W. R. Wilde, p. 261.)

POLYPI OF THE EAR.—The treatment of polypi by extraction, or touching them with the nitrate of silver, is generally useless. The best application is a thin stick of potassa cum calce prepared by Mr. Bailey, of Wolverhampton, which, as it contains a little iron, is firmer and harder, and it deliquesces much less rapidly than when prepared in the usual way. The external meatus having been syringed with tepid water so as to remove the whole of the discharge from the surface of the polypus, as well as from that of the meatus, the tube and polypus should be dried by a portion of fine cotton wool, attached to the end of a probe; a portion of glass tube, about an inch and a half long, should then be selected,—the diameter of the meatus,—and passed onwards as far as the polypus, when by gentle pressure a portion of the free extremity of the polypus is made to protrude into the interior of the tube, and is surrounded by it. Upon looking into the tube and re-

certaining that the polypus is embraced by its inner extremity, the tube is steadied by the left hand, and with the right a portion of the potassa cum calce is passed inwards, and gently pressed against the polypus. The immediate effect of the application of the potassa cum calce upon the surface of the polypus is to change its colour from a bright red to a livid hue, and this takes place without any pain being experienced by the patient, if the meatus has been completely guarded. After the application has been made, the patient should sit still for three or six minutes, and the tube be allowed to remain as it was fixed during the operation. The meatus is now to be syringed out with tepid water, when blood mixed with *débris* of the polypus will come away. The surface of the polypus still remains of a dark colour, and during several hours a process of slow dissolution takes place in all that portion which the escharotic has reached. If the polypus is too dry, the escharotic must be moistened, and if pain should be felt at that time of the application, or afterwards, the ear should be immediately syringed with tepid water. (Mr. J. Toynbee, p. 253.)

Globular Vascular.—Apply an astringent solution of zinc, accetate of lead, alum, or tannin, to the meatus, after the ear has been washed out by the syringe, and let it be retained for half an hour. This is to be used twice or thrice a day, taking care to remove the sediment of the last application before again using the solution. A slight discharge from the mastoid process is valuable, as preventing congestion of the tympanic cavity during the use of the drops. (Mr. J. Toynbee, p. 257.)

Ophthalmia Tarsi.—Let ung. picis be rubbed carefully into the roots of the tarsi at bed-time, and washed away with castile soap and tepid water in the morning. (Dr. Parrish, p. 252.)

Pupil, *Artificial.*—[For a new method of operating in these cases, and for the description of a new scissors for this purpose, introduced by Mr. Bowman, see p. 250.]

Strumous Ophthalmia.—Use one part of Scheele's acid with two of distilled water. It may, in some instances, be used in a more concentrated form. A few drops to be applied to the eye night and morning. (Mr. J. V. Solomon, p. 252.)

MIDWIFERY, AND THE DISEASES OF WOMEN.

Dysmenorrhœa.—Although this is frequently a symptom of uterine disease, we are often obliged to treat it as the chief disease. If warmth and rest fail, inject from 15 to 20 minims of laudanum, mixed with a little warm water, into the rectum: this is of more value than double the quantity by the mouth, and its disagreeable effects are avoided. If the first injection fails, or is not retained, a second, half an hour later, will be more successful. Chloroform, either inhaled or given in doses of 30 or 40 minims, mixed with mucilage or camphor, which favours its solution in water, is sometimes of use, though not so valuable as opiates. (Dr. Henry Bennett, p. 296.)

Fibrous Tumour *of the Labium.*—In a case of this nature, in Guy's Hospital, Mr. Massey made an incision in a longitudinal direction from one extremity of the labium to the other to the inner side of the large venous trunk, and just within the mucous surface. A finger was then introduced between the integuments and tumour, and its loose connections with the surrounding tissues forcibly torn through, and the mass completely enucleated. It is of some practical importance to remember that these tumours are closely coated with cellular tissue of a dense character, and it is necessary to cut through this in order to turn out the growth. It is quite easy for the operator to mistake the capsule for the surface of the tumour, and to be trying to separate the integument from it, which would be a long process; and to prevent this it is better to cut slightly into the tumour itself, when the edge of the capsular covering is at once indicated, from whence the enucleation is readily completed. (Mr. A. Massey and Dr. Oldham, p. 290.)

Galvanism, *in Obstetric Practice.*—Do not employ the apparatus in which two currents are produced, but simply the single-current machine, for a direct current *tends to* produce contraction, an inverse current, paralysis. In using the single-*current machine*, place the positive conductor over the lumbo-sacral region, and

carry the other over the abdominal surface, with a gentle friction. In this way powerful uterine contractions may be easily excited. (Dr. G. Bird, p. 278.)

Head-Presentation, *Turning in.*—On this subject, Dr. Ramsbotham says, the following are the circumstances only under which he would resort to this operation:—If the clear available space in the conjugate diameter were about 3¼ inches, or from that to 3½—if the woman's children had all previously been born dead—if the membranes were still whole, or the liquor amnii having been evacuated, the uterus had not contracted closely round the child's body, the head being perfectly free above the pelvic brim, not having as yet descended into the pelvic cavity; and if the attendant, by being in the habit of performing obstetrical operations, had acquired a certain dexterity in regard to them, and had perfect confidence in himself. (Dr. Ramsbotham, p. 276.)

Ovarian Section.—[On the merits and demerits of this question, the conclusions arrived at, after attentive consideration of the subject by the reviewer in the Brit. and For. Medico-Chirurg. Review, will be found at p. 289.]

Polypus Canula.—[For a description of a new instrument employed by Dr. Oldham, for the purpose of removing a large polypus from the uterus, see p. 286.]

Sponge Tents.—[Dr. Rigby describes a mode of making sponge-tents in the 'Medical Times' of the 6th of December. This method was described in the 'Retrospect' (see vol. xxii.,) at the instance of Mr. Charles Coates, then of Leeds, now house-surgeon to the Bradford Infirmary, p. 294.

Turning.—Chloroform is very valuable in cases where turning is required in diminishing the spasmodic efforts of the uterus—succeeding when opium had failed, or where it was inadmissible. (Dr. J. H. Davies, p. 277.)

MISCELLANEA.

Anæsthesia, *Arrest of.*—Anæsthesia is a momentary disturbance of the cerebro-spinal system. To remedy it—apply electricity to various parts of the body, especially over the cerebro-spinal axis. (Dr. Abeille, p. 328.)

Arsenic, *Test for.*—Give magnesia mixed to the consistence of cream, in doses of two or three tablespoonfuls. In a case reported by Dr. Maclagan, the patient took four ounces in three-quarters of an hour, and no doubt vomited it nearly all up again. Reinsch's test is a most valuable and convenient one in cases of poisoning by arsenic. All that is required is a little muriatic acid, a bit of copper wire, and some vessel as a test tube, in which they can be heated together. These articles are so common that it must be rare when they cannot be supplied. If the piece of copper be crusted black during the boiling, and when heated in a candle flame, loses its crust, and gives off alliaceous fumes, the evidence will be sufficiently precise. (Dr. D. Maclagan, p. 322, and Dr. H. Rainy, p. 341.)

Chloroform.—Never administer it to a patient with a full stomach. Take particular care the vapour is diluted with plenty of atmospheric air, and never proceed to stertorous respiration if it can be avoided. Operate as soon as the patient becomes insensible to sound, or the pupils dilate. Death from chloroform takes place in consequence of the density of the air mixed with the heavy vapour of the chloroform being nearly equal to or greater than that of the carbonic acid to be expelled from the blood. The nearer the two densities correspond the less carbonic acid escapes from the blood, and the function of respiration no longer goes on. The specific gravity of carbonic acid is 1·523; the specific gravity of atmospheric air at 60°, saturated with the vapour of chloroform, is 1·355, and is therefore perfectly respirable: but if we increase the temperature of the air to 70° it will take a much larger quantity of the vapour, by which the specific gravity will be increased to 1·533. If this is neglected danger will ensue. In restoring a patient from the influence of chloroform, galvanism is the only chance. A current of electricity must be kept up through the fifth nerve, medulla oblongata, phrenic nerves and diaphragm, as long as respiratory movements can be produced, and let the patient have plenty of fresh air or oxygen gas, and the case must do well, for the blood will remain fluid for a long time, and circulation will go on as long as respiration can be carried on artificially. (Dr. W. B. Herapath, p. 318.)

Test for.—When a few drops are rubbed between the palms of the hands, a fragrant odour is emitted, in no way pungent. If there is any pungent odour present, we may suspect its impurity. (Dr. Murphy, p. 328.)

Dr. Arnott's Freezing Process.—Place equal parts of salt and well-pounded ice in a gauze bag, having the margins attached to a gutta percha ring. By gently touching the part to be rendered insensible for a minute or two with the bottom of the bag, the surface becomes suddenly frozen, and the pain of course disappears. To obviate the tingling sensation which is apt to ensue upon the return of sensibility, ice without salt is to be used, and thus, no uneasiness whatever is experienced. (Dr. J. Arnott, pp. 299, 300.)

Head, *Cold in the.*—It is a practical fact, not half so generally known as it deserves to be, that the common cold in the head, however severe, may be at once relieved by guaiacum. (Dr. R. H. Goolden, p. 63.)

Laudanum, *Poisoning by.*—In a case of poisoning by laudanum, in an infant, respiration was excited by galvanism, by placing the zinc or positive wire on the mucous membrane of the mouth, and the negative or copper wire just below the ensiform cartilage. The current seemed to enter by the fifth nerve, from this to the medulla oblongata, then along the phrenic and external respiratory and spinal nerves to the diaphragm, and the intercostal and other accessory muscles of the respiratory apparatus. If the positive or zinc wire slipped from the cheek to the tongue, the movements became more gasping and convulsive—another set of nerves becoming influenced. (Dr. W. B. Herapath, p. 317.)

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